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### **On the concept of experimental error.**

Hon, Giora

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ON THE CONCEPT OF EXPERIMENTAL ERROR

by

Giora Hon

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Department of History and Philosophy of Science,

Chelsea College, London University

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To Hannah Safran

## ABSTRACT

This thesis is concerned with the concept of experimental error. It considers the occurrence of error an epistemological phenomenon which stands in the way of attaining knowledge. The concept of experimental error comprises intertwined elements of conceptual and physical origins. These elements form an obstacle that may obstruct the experimenter from reaching his goal, that is, the attainment of knowledge. The prime objective of the thesis is to throw light on such obstacles. It questions the conclusiveness of experimental results by exposing the conceptual and physical difficulties which attend the execution of an experiment. However, the thesis underlines the positive feature of this concept, namely, that the comprehension of an error may contribute to knowledge.

The thesis holds that the mathematical theory of error avoids conceptual discussions, and conceals the physical elements of experimental error under the mathematical cloak of abstractions. The mathematical theory of error is solely concerned with rendering the experimental error amenable to mathematical analysis and calculation. As the thesis seeks a comprehensive view of the concept of experimental error, it does not follow the mathematical approach.

By way of introduction, the thesis juxtaposes several approaches towards the epistemological problem of error. It outlines the views of Aristotle, Bacon, Descartes, Spinoza and others, and concludes with Wittgenstein's instructive suggestion of distinguishing between the different ways in which something 'turns out wrong'. The thesis then focuses on the problem of experimental error by contrasting the approach of the Greek astronomers towards this problem with that of Kepler. An account of Kepler's explicit and comprehensive awareness of the problem of experimental error - an account which includes a study of his *Astronomia Nova* and his work on optics - serves as a background against which the thesis introduces a classification of different types of experimental error. This classification consists of four categories: background theory; assumptions concerning the actual set-up and its working; observational reports; and theoretical conclusions.

The thesis then proceeds to illustrate the classification with several case-studies of experiments. It deals mainly with the following: R.A. Millikan's and F. Ehrenhaft's measurements of the charge of the electron; H. Hertz's experiment on the deflection of cathode rays in an electric field; R. Blondlot's so-called discovery of N rays; and J. Franck's and G. Hertz's experiment on the quantized spectrum of the atom's energy levels. In the final section, the thesis presents Kaufmann's experiment in its historical setting and contrasts some of the principal responses it received. The varied responses illustrate how the detection of an experimental error and its characterization depend on the philosophical outlook one holds.



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Once [errors of measurement and other forms of experimental error]... have been discounted, our attention can turn to the logico-mathematical structure.<sup>1</sup>

W. Sellars

There is no such thing as a classification of the ways in which men may arrive at an error; it is much to be doubted whether there ever can be.<sup>2</sup>

A. de Morgan

Knowledge and error flow from the same mental sources, only success can tell the one from the other. A clearly recognized error, by way of corrective, can benefit knowledge just as a positive piece of knowledge can.<sup>3</sup>

E. Mach

The history of science is not restricted to the enumeration of successful investigations. It has to tell of unsuccessful inquiries, and to explain why some of the ablest men have failed to find the key of knowledge, and how the reputation of others has only given a firmer footing to the errors into which they fell.<sup>4</sup>

J.C. Maxwell

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1. Sellars, 1961, p.73; quoted by Mellor, 1965, p.106.

2. De Morgan, 1847, p.237.

3. Mach, 1976, p.84.

4. Maxwell, 1890, p.251.

## PREFACE

In his book, Discovering Plato, Alexandre Koyré (1892-1964) explicates, inter alia, the philosophical arguments which Plato employs in the Theaetetus to reveal the nature of knowledge and, for that matter, science.<sup>1</sup> The characters of the dialogue are ideal interlocutors for a scientific discussion: Theodorus is a good mathematician and astronomer, and Theaetetus is a teacher in the Academy and one of the leading geometers of his time. Having stated that wisdom and knowledge are the same, Socrates admits that he is not quite clear as to what knowledge really is. 'Herein lies the difficulty which I can never solve to my satisfaction,' Socrates confesses and proceeds to question Theaetetus: 'What is knowledge? Can we answer that question?'<sup>2</sup> Needless to say, the great expertise of the interlocutors does not stand them in good stead, and they fail at the end of the dialogue to arrive at a satisfactory solution. 'And so, Theaetetus,' Socrates rhetorically asks, 'knowledge is neither sensation nor true opinion, nor yet definition and explanation accompanying and added to true opinion?'<sup>3</sup> A view with which Theaetetus has no choice but to agree. And thus another of Plato's dialogues comes to an end in a Socratic manner, that is, as Socrates himself concludes, 'if...you should ever conceive afresh, you will be all the better for the present investigation, and if not, you will be soberer and humbler and gentler to other men, and will be too modest to fancy that you know what you do not know'.<sup>4</sup>

In the course of explaining the second thesis of Theaetetus, namely, that knowledge is true opinion, Koyré, following Socrates, points out that

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1. Koyré, 1945, pp.33-52.

2. Plato, 1949, p.5.

3. Ibid., p.84.

4. Ibid.

it amounts to the claim that errors are impossible. Having said that, he relegates the following sentence to a footnote: 'The problem of error,' Koyré states, 'is one of philosophy's very serious and crucial problems.'<sup>1</sup> This emphatic statement of Koyré, which stresses the importance of the problem of error in a rather incidental way, may serve as the departure point of this thesis. Indeed, Koyré's remark epitomizes the state of the problem of error: it is 'very serious and crucial', yet the treatments it has received have generally been scanty and peripheral, that is, metaphorically they amount to a footnote.

In this thesis I propose to promote the status of the problem of error from a footnote to a central issue. As a student of philosophy of science, my principal concern will be to focus attention on this issue in science, specifically on the problem of experimental error.

Although this problem poses great difficulties, philosophers of science tend generally to disregard it; their concern with the analyses of theories and their logical structures suppresses the physical issues and the concomitant practical problems which arise in the attempt to test theoretical claims experimentally. Scientists who are directly confronted with this problem, pay it lip service; they rely entirely upon the mathematical theory of error whose inherent dichotomy of systematic and random errors is at times defective and at times misleading.

The mathematical theory of error defines error in such a way so that it can dispense with the conceptual elements of error and conceal the related physical difficulties under the mathematical cloak of abstractions. The prime objective of that theory is to render the error amenable to mathematical analysis and calculation. This is however too narrow an approach to be followed by a thesis which seeks a comprehensive view of the concept of experimental error.<sup>2</sup>

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1. Koyré, 1945, p.40, footnote no.9.

2. The mathematical approach will be discussed in Chapter IV.

Taking a broad view, an error can be considered an epistemological phenomenon which stands in the way of attaining knowledge. In this sense, an error may be represented as an obstacle which obstructs and diverts the search for knowledge; it thus may occur in every human activity which pursues and displays knowledge. Indeed, as Hobbes observed, 'error occurs not only in affirming and denying something, but also in feeling, and in men's silent thinking'.<sup>1</sup> However, since my principal concern is scientific knowledge - a knowledge which has the features of communicability and testability - I shall limit myself to errors which find their expressions in statements. Moreover, as this study focuses upon errors which may arise in the process of executing an experiment, that is, experimental errors, I shall be mostly concerned with statements pertaining to experiments. In this limited sense, an error occurs when a statement fails to represent the correct state of the system under examination; a statement to which however one commits oneself as a result of a certain judgement.

It should be noted that I assume complete honesty on the part of the experimenter. I shall not be dealing with the phenomena of lying and deceit; the ethics of the scientific enquiry being separable from epistemological problems.

In the method of experimentation the system under examination is physical; the execution of an experiment always involves a certain conceptual framework within which a physical system is prepared and studied. Thus, the related errors have both conceptual and physical elements. In this thesis I attempt to give a comprehensive account of the concept of experimental error by examining both elements.

The thesis consists of six chapters. In the Introduction I delineate various views of the general epistemological problem of error. I then

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1. T. Hobbes, Computatio sive Logica, Ch.V. Quoted by Mill, 1843, p.332.

focus - in Chapters II and III - on the problem of experimental error by contrasting the approach to this issue in Greek science with the view Kepler took. Against this background I introduce, in Chapter IV, a classification of types of experimental error which, in Chapter V, is followed up for the purpose of illustration with case-studies of experiments. In the final chapter, I show how the characterization of an experimental error depends on one's philosophical outlook; I thereby return to the general epistemological issue. I conclude the thesis in the Socratic spirit with an emphasis on the shortcomings of the claim to knowledge. Thus, the original Latin root of the term 'error', namely, erro, takes on significance: wandering, going astray - being on the way without ever stopping, without ever arriving.

#### Coda

The poet W.H. Auden observed that 'drama is based on the Mistake. I think someone is my friend when he really is my enemy, that I am free to marry a woman when in fact she is my mother, that this person is a chambermaid when it is a young nobleman in disguise.... All good drama', Auden concluded, 'has two movements, first the making of the mistake, then the discovery that it was a mistake'.<sup>1</sup>

The task of the student of philosophy of science is to examine the scientific scene and to review critically the drama of science which has been enacted throughout the ages. Carrying out this task the student finds quite quickly that this drama is lacking neither errors nor their discoveries; like any other human pursuit it is rather rich with them. For, as Seneca observed, 'among the other inconveniences of mortality, there is also this one, the fog of minds: not only the inevitability of erring, but even a love of errors'.<sup>2</sup>

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1. Auden, 1975, p.471.

2. Seneca, *Ira*, II, cap.9. Quoted by Montaigne, 1948, p.142 (Bk.II, Ch.xii), my translation.

To clear this fog we must shed light on the source of error. In other words, as Wittgenstein put it, 'one must reveal the source of error, otherwise hearing the truth won't do any good. The truth cannot force its way in when something else is occupying its place... one must find the path from error to truth'.<sup>1</sup>

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1. Wittgenstein, 1979, p.61.

## CHAPTER I

### Introduction

The setting of the general problem of error takes shape in a contrariety that can be discerned in the realm of epistemology. A polarity appears to exist between, on the one hand, the claim to knowledge as certainty and, on the other, the inherent fallibility of the human faculties which are supposed to attain knowledge. The admission that fallibility inheres in the process of attaining knowledge may force us to consider knowledge uncertain. Thus, if we persist in the claim to knowledge as certainty, we are then required - having admitted fallibility - to address ourselves to the problem of error: to examine error as an obstacle which stands in the way of knowledge, and to account for its occurrence. It is the tension that pervades this contrariety that furnishes the problem of error with significance.

Past philosophers of grand systems - systems which they designed to provide safe routes to knowledge - were required by the very systems they built to analyse the epistemological phenomenon of error. After all, a system of philosophy which claims to arrive at knowledge has to guard against the possibility of falling into error, for the spectre of errors in the journey to knowledge is permanent. One therefore expects to find such philosophers engaged in attempting to solve this problem or at least to address it and give it its due considerations. However, this is not generally the case; moreover, when the problem is treated it is perceived from the epistemological stand of the system itself and, in accordance with his doctrine, the philosopher contrives ways to dissolve the problem and secure the path to knowledge. As the following examples illustrate, the problem of error appears intractable. With these examples I intend to outline the general epistemological problem of error. I do not therefore seek



depth but rather breadth. The juxtaposition of the following different approaches to the general problem of error, forms a background against which I shall study the specific problem of experimental error.

The work of Aristotle has been hailed as the first generalized programme of inquiry, including empirical research, into nature;<sup>1</sup> it thus seems appropriate to start my general outline of the problem of error with Aristotle's view of this problem. In his theory of sense-perception, Aristotle distinguishes between direct perception by an individual sense of its own specific sensible and the perception of the common sensibles when more than one sense may be involved. Aristotle observes that there are special objects of sense which can trigger only that one particular sense. These objects of sense cannot be detected in any of the senses other than that one, 'in respect of which,' Aristotle claims, 'no error is possible; in this sense colour is the special object of sight, sound of hearing, flavour of taste. Touch, indeed, discriminates more than one set of different qualities'.<sup>2</sup> In his view, 'each sense has one kind of object which it discerns, and never errs in reporting that what is before it is (e.g.) colour or sound'.<sup>3</sup> Hence, the individual senses, according to Aristotle, always perceive their specific sensibles truly. In other words, the perception of the specific sensibles is immune to error. On this view, there would have been no error if the account of the physical world were to be based solely on the perception of the specific sensibles.

However, although the individual sense never errs in reporting its specific sensibles, it may still err, Aristotle holds, 'as to what it is that is coloured or where that is, or what it is that is sounding or where

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1. Lloyd, 1979, p.225.

2. Aristotle, *De Anima*, 1963, 418<sup>a</sup>12-14. The specific sensibles of touch are texture, rigidity and heat.

3. Ibid., 418<sup>a</sup>15-16.

that is'.<sup>1</sup> The senses, in other words, cannot be in error about their special objects, but they are liable to error in dealing with the common sensibles.<sup>2</sup> Thus, as Aristotle writes, 'while the perception that there is white before us cannot be false, the perception that what is white is this or that may be false'.<sup>3</sup> Errors therefore may occur in the process of determining for instance, movement, rest, number, figure, or magnitude.<sup>4</sup>

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1. Ibid., 418<sup>a</sup>16-17.

2. At only one instance does Aristotle relax this strong claim. He writes that 'perception of the special objects of sense is never in error', and adds almost parenthetically, 'or admits the least possible amount of falsehood'. (Ibid., 428<sup>b</sup>18-19.) This qualification suggests that there is after all room for error, no matter how slight, in the perception of the specific sensibles. If one were to take the logical standpoint, that is, that our experience cannot be otherwise than what it is, regardless of our ability to report that experience, then there would have been absolutely no room for error. To explain Aristotle's qualifying remark, I. Block, in his paper, 'Truth and Error in Aristotle's Theory of Sense Perception', takes the teleological standpoint, that is, that 'the individual senses were fashioned by Nature specifically for perceiving these sensibles'. (Block, 1961, p.7.) Aristotle's concept of 'for the most part', which distinguishes between normal and abnormal conditions, is an essential element of this view that can explain away all exceptions. Thus, as Block writes, 'when Aristotle says that perception of the specific sensibles is always true, he means as long as the sense is functioning normally under normal conditions'. (Ibid., pp.7-8.) However, according to Aristotle, the perception of the common sensibles can be in error even under normal conditions. Hence, the perception of the specific sensibles, which can be in error only in instances of abnormal conditions, is said to have the least amount of error in comparison with the perception of the common sensibles. Thus, Block explains, 'when Aristotle says that we always perceive the specific sensibles truly, he means by "always", always under normal conditions'. (Ibid., p.8.) Cf. Wittgenstein, 1977, p.6e §27.

3. Aristotle, De Anima, 1963, 428<sup>b</sup>21-2.

4. Ibid., 418<sup>a</sup>18; these are examples of common sensibles which Aristotle gives. A. Kenny puts forward a different interpretation altogether. In his paper, 'The Argument from Illusion in Aristotle's Metaphysics (I, 1009-10)', he argues that for Aristotle the infallibility of the senses about their special objects does not mean that whatever they report is true. Kenny claims that 'statements such as "That is red" made on the basis of visual experience are not incorrigible. What is special about them', Kenny points out, 'is that they can be corrected only by a further use of the same sense'. (Kenny, 1967, p.193.) Thus, according to Kenny, the distinction which Aristotle draws is not

(cont. on p.14)

Aristotle clearly maintains the distinction between sensation and appearance; whatever he may be, he is not a sensationalist. 'Even if sensation - at least of the object peculiar to the sense in question - is not false, still', Aristotle holds, 'appearance is not the same as sensation'.<sup>1</sup> For Aristotle, 'things do not appear either the same to all men or always the same to the same man, but often have contrary appearance at the same time'.<sup>2</sup> It is here, in the transition from sensation to appearance, that errors originate. As Aristotle puts it, 'in forming opinion we are not free: we cannot escape the alternative of falsehood or truth'.<sup>3</sup>

Aristotle rightly criticizes his predecessors who failed to take account of errors; for in his view, 'the soul continues longer in the state of errors than in that of truth'.<sup>4</sup> Since they looked upon thinking as a bodily process like perceiving, and held that like is known as well as perceived by like, they could not escape, Aristotle asserts, the following dilemma: 'Either (1) whatever seems is true... or (2) error is contact with the unlike; for', Aristotle explains, 'that is the opposite of the knowing of like by like'.<sup>5</sup> In Aristotle's own scheme the dilemma does not arise since in his view there is no purely physical account which can explain the occurrences of errors, nor the attainment of knowledge. Indeed, for Aristotle, it is a received principle that 'error as well as knowledge

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(f/n.4 cont.from p.13)

between an infallible and fallible mode of perception, but rather between different methods of correction: to correct the perception of the common sensibles one can appeal to the judgements of different senses, but in the case of the specific sensibles, the individual senses, though corrigible, can be corrected only by themselves - they are, in a word, the sole and final judges with respect to the specific sensibles. (Ibid., pp.193-4.)

1. Aristotle, Metaphysica, 1963, 1010<sup>b</sup>1-3.
2. Ibid., 1011<sup>a</sup>31-33.
3. Aristotle, De Anima, 1963, 427<sup>b</sup>20.
4. Ibid., 427<sup>b</sup>1.
5. Ibid., 427<sup>b</sup>2-4; cf. 427<sup>a</sup>16-30.

in respect to contraries is one and the same'.<sup>1</sup> In other words, the two concepts complement each other; if the coin of philosophy bears on its obverse 'Knowledge', then 'Error' should be impressed on the reverse.

Aristotle acknowledges therefore that the possibility of erring inheres in the process of attaining knowledge. However, he provides first principles which form in his view a sufficiently reliable guideline along which knowledge can be attained.<sup>2</sup> Francis Bacon (1561-1626), in contrast, rejects these very principles on the ground that they constitute in themselves a self-perpetuating error. Perhaps he would have liked to say that Aristotle minted a counterfeit.

Bacon argues in his celebrated Novum Organum, that Aristotle 'has corrupted Natural Philosophy with his Logic; ... he has made the Universe out of Categories'.<sup>3</sup> In Bacon's view, the application of Aristotle's doctrine has rather the effect of confirming and rendering permanent errors which are founded on vulgar conceptions, than of promoting the investigation of truth.<sup>4</sup>

Bacon builds his own programme on the doctrine that truth is manifest through plain facts. The student of nature should get rid of all his prejudices and preconceived ideas: 'the whole work of the mind should be recommenced anew';<sup>5</sup> only then would he experience things as they are, i.e., the truth. 'Our plan', Bacon explains, 'consists in laying down degrees of certainty, in guarding the sense from error by a process of correction...

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1. Ibid., 427<sup>b</sup>5. Cf., Hamlyn, 1968, p.130. See also Mach's view in the mottoes of this thesis.

2. To know is to know by means of causes. (Aristotle, Analytica Posteriora, 1963, 71<sup>b</sup>9-12, 94<sup>a</sup>20; Physica, 1966, 184<sup>a</sup>10-14.) On the four causes see, e.g., Ross, 1974, pp.71-75.

3. Bacon, 1859, p.39 (Bk. I, lxiii).

4. Ibid., pp.13-14 (Bk. I, xii).

5. Ibid., p.4 (Preface).

and then in opening and constructing a new and certain way for the mind from the very perceptions of the senses'.<sup>1</sup> In this way, Bacon concludes, 'we are building in the human Intellect a copy of the universe such as it is discovered to be, and not as man's own reason would have ordered it'.<sup>2</sup> Thus the first task of the scientist is to eliminate errors from his cognition by the 'expiation and purgation of the mind',<sup>3</sup> and only then can he enter 'the true way of interpreting Nature'.<sup>4</sup>

Bacon finds it necessary therefore to expound in considerable detail the subject of the obstacles to truth, before proceeding to unfold his method. He devotes nearly the whole of the first book of Novum Organum to the examination of these obstacles which he calls idols. This name reflects the Platonic concept of eidolon (εἶδωλον), which refers to a fleeting, transient, image of reality, in contrast to the concept of idea (ἰδέα), which represents reality in the Platonic sense.<sup>5</sup> Although Bacon admits that 'to draw out conceptions and axioms by a true induction is certainly the proper remedy for repelling and removing idola';<sup>6</sup> he still finds it of great advantage to indicate the idols. For, as he explains, 'the doctrine of idola holds the same position in the interpretation of Nature, as that of the confutation of sophisms does in common Logic'.<sup>7</sup>

Bacon classifies four types of idols which, as he puts it, 'beset the

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1. Ibid., p.3 (Preface).

2. Ibid., p.120 (Bk.I, cxxiv).

3. Ibid., p.51 (Bk.I, lxix).

4. Ibid.

5. Ibid., pp.16-17, footnote. Cf. Adamson and Mitchell, 1910-1911, p.146, footnote no.4.

6. Bacon, *ibid.*, p.21 (Bk.I, xl).

7. Ibid., *emphases in the original.*

minds of men'.<sup>1</sup> The first kind are the idols of the tribe; that is, errors incidental to human nature in general. Of these, the most prominent are the tendency to support a preconceived opinion by affirmative instances, whilst neglecting all counter examples; the tendency to generalize from a few observations, and to consider mere abstractions as reality. Errors of this type may also originate in the weakness of the senses, which affords scope for mere conjectures.<sup>2</sup> Bacon warns the student of Natural Philosophy against the belief that the human sense is the measure of things; he insists upon the awareness that 'all perceptions, both of sense and also of mind, are referred to man as their measure, and not to the universe'.<sup>3</sup> For Bacon, 'the human intellect is like an uneven mirror on which the rays of objects fall, and which mixes up its own nature with that of the object, and distorts and destroys it'.<sup>4</sup>

The second kind of idols are the idols of the cave; that is, errors incidental to the peculiar mental and bodily constitution of each individual (the cave is a direct reference to Plato's simile in the Republic). These errors may be either of internal origin, arising from the peculiar physiology of the individual, or of external origin, arising from the social circumstances in which one is placed by education, and society in general.<sup>5</sup>

The third class of idols comprises idols of the market-place; that is, errors arising from the nature of language.<sup>6</sup> Language, according to

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1. Ibid., p.21 (Bk.I, xxxix).

2. Ibid., pp.21,24-9 (Bk.I, xli,xlv-ly). Cf., Adamson and Mitchell, 1910-1911, p.146.

3. Bacon, *ibid.*, p.21 (Bk.I, xli).

4. Ibid. For a similar metaphor and another discussion of the idols, see Bacon, 1950, pp.132-35.

5. Bacon, 1859, pp.22, 29-30, 32-3 (Bk.I, xlii, liii, lviii). Cf., Adamson and Mitchell, 1910-1911.

6. Bacon, *ibid.*, pp.22-3, 33-5 (Bk.I, xliii, lix, lx). Cf., Adamson and Mitchell, *ibid.*

Bacon, introduces two fallacious modes of observing the world. First, there are some words that are merely 'the names of things which have no existence (as there are things without names through want of observation, so there are also names without things through fanciful supposition)'.<sup>1</sup> Secondly, there are 'names of things which do exist, but are confused and ill defined'.<sup>2</sup>

The fourth and last class consists of the idols of the theatre; that is, errors arising from received 'dogmas of philosophical systems, and even from perverted laws of demonstrations'.<sup>3</sup> Here Bacon mainly refers to three kinds of error: sophistical, empirical and superstitious. The first error corresponds to Aristotle's system which, according to Bacon, forces nature into an abstract scheme and makes 'the Universe out of Categories'.<sup>4</sup> The second error, the empirical, refers to the jumping from 'narrow and obscure experiments' to general conclusions (Bacon has in mind particularly the chemists of his time and the experiments of Gilbert);<sup>5</sup> and the third error, the superstitious, represents the corruption of philosophy by the introduction of poetical and theological notions, as is, according to Bacon, the case with the pythagorean system.<sup>6</sup>

Concluding his discussion of the idols, Bacon demands that all of them 'must be renounced and abjured with a constant and solemn determination'.<sup>7</sup> He insists upon freeing and purging the intellect from them, so that 'the

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1. Bacon, *ibid.*, p.34 (Bk.I, lx).

2. *Ibid.*

3. *Ibid.*, p.23 (Bk.I, xliv). Cf., *ibid.*, pp.35-49 (Bk.I, lxi-lxvii); Adamson and Mitchell, 1910-1911.

4. Bacon, *ibid.*, p.39 (Bk.I, lxiii).

5. *Ibid.*, pp.41-2 (Bk.I, lxiv).

6. *Ibid.*, pp.42-4 (Bk.I, lxv).

7. *Ibid.*, p.49 (Bk.I, lxviii).

approach', as he describes his quest, 'to the Kingdom of Man, which is founded on the Sciences, may be like that to the Kingdom of Heaven'.<sup>1</sup>

It is striking to observe with what ease Bacon rejects other possible errors which can render his own doctrine erroneous. 'It will doubtless occur to some', Bacon remarks, 'that there is in the Experiments themselves some uncertainty or error; and it will therefore, perhaps, be thought that our discoveries rest on false and doubtful principles for their foundation'.<sup>2</sup> This is indeed an important remark with which the concepts of prejudice and preconceived idea cannot deal. But Bacon dismisses it off hand; 'this is nothing', he exclaims, 'for it is necessary that such should be the case in the beginning'. Analogically, he explains that

it is just as if, in writing or printing, one or two letters should be wrongly separated or combined, which does not usually hinder the reader much, since the errors are easily corrected from the sense itself. And so men should reflect that many Experiments may erroneously be believed and received in Natural History, which are soon afterwards easily expunged and rejected by the discovery of Causes and Axioms.<sup>3</sup>

Finally, Bacon assures the student that he should not 'be disturbed by the objections which we have mentioned'.<sup>4</sup>

Notwithstanding Bacon's resolute assurance, the objections are disturbing; for it is precisely this very sense - the sense which according to Bacon's analogy is given - that science lacks. Bacon would have us believe that the analogy between a printer's error and an experimental

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1. Ibid.

2. Ibid., pp.111-12(Bk.I, cxviii).

3. Ibid., p.112 (Bk.I, cxviii).

4. Ibid. Bacon admits however that 'it is true, that if the mistakes made in Natural History and in Experiments be important, frequent, and continuous, no felicity of wit or Art can avail to correct or amend them'. (Ibid.) Thus, if there lurked at times, as Bacon puts it, 'something false or erroneous' in the particulars of his Natural History [which he claims to have proved with 'so great diligence, strictness, and', Bacon is prepared to add, 'religious care'], what then must be said, Bacon asks rhetorically, 'of the ordinary Natural History, which, compared with ours, is so careless...? or of the Philosophy and Sciences built on such sands, or rather quicksands?' (Ibid.) It is in this context of comparison that Bacon brings forth his assurance.



error is faithful; but, as I shall claim later, these two types of error are categorically distinct.

Undoubtedly, some sources of errors can be identified as either prejudices or preconceived ideas; however, to claim that the sources of all errors are prejudices and preconceived ideas, is an error in itself, which in Bacon's case has indeed become a prejudice.<sup>1</sup>

Bacon, as is well known, had professed a new methodological outlook; René Descartes (1596-1650), his junior contemporary, went further afield and inaugurated an epistemological revolution. Although both adhered to the doctrine that truth is manifest, their views diverged as to the form it takes. Whereas for the empiricist Bacon truth is manifest through plain facts, for the rationalist Descartes it is plain reasoning which exhibits truth.

In his Meditations, Descartes assumes the impossibility that God should ever deceive us. He argues that 'in all fraud and deception some imperfection is to be found, ... the desire to deceive without doubt testifies to malice or feebleness, and accordingly cannot be found in God'.<sup>2</sup> God is benevolent and He, therefore, does not lie to us. 'As He could not desire to deceive me', Descartes observes, 'it is clear that He has not given me a faculty that will lead me to err if I use it aright'.<sup>3</sup> Yet, 'experience shows me', he admits, 'that I am nevertheless subject to an infinitude of errors'.<sup>4</sup> Hence the problem: God is benevolent and yet we do err as if God has deceived us.

This formulation of the problem of error puts error on a par with sin. In this view, all errors can in principle be avoided, and so Descartes

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1. Agassi, 1975, p.67.

2. Descartes, Vol.1, 1973, p.172 (Meditation IV).

3. Ibid.

4. Ibid.

can claim with certainty 'that God could have created me so that I could never have been subject to error'.<sup>1</sup> Descartes is therefore concerned less with investigating the nature of error for its own sake, than with devising practical steps for its avoidance.<sup>2</sup>

Descartes' first explanation that error arises because the power God has given man for the purpose of distinguishing truth from error is not infinite, does not satisfy him.<sup>3</sup> This kind of explanation does not allow for the possibility of avoiding absolutely all errors. Conformably to his method, he proceeds to examine himself more closely. Considering his own errors, he observes that 'they depend on a combination of two causes. to wit, on the faculty of knowledge that rests in me, and on the power of choice or of free will - that is to say, of the understanding and at the same time of the will'.<sup>4</sup>

Having established the two basic components involved in erring, he describes its mechanism. Errors, he explains,

come from the sole fact that since the will is much wider in its range and compass than the understanding, I do not restrain it within the same bounds, but extend it also to things which I do not understand: and as the will is of itself indifferent to these, it easily falls into error and sin, and chooses the evil for the good, or the false for the true.<sup>5</sup>

According to Descartes' solution, one can never fall into error as long as one restrains one's will within the limits of one's knowledge, and forms no judgement except on matters which one's understanding clearly and distinctly conceives. 'It is in the misuse of the free will', Descartes concludes, 'that the privation which constitutes the characteristic nature of error is met with'.<sup>6</sup> Furthermore, 'it is without doubt an imperfection

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1. Ibid., p.173 (Meditation IV).

2. Evans, 1963, p.136.

3. Descartes, op.cit.

4. Ibid., p.174 (Meditation IV).

5. Ibid., pp.175-6 (Meditation IV).

6. Ibid., p.177 (Meditation IV).

in me', Descartes admits, 'not to make a good use of my freedom, and to give my judgement readily on matters which I only understand obscurely'.<sup>1</sup> There is indeed no compulsion to assent to what is unclear, for there is always the possibility to suspend judgement. Hence, for Descartes, 'whenever we err there is some fault in our method of action, or in the manner in which we use our freedom; but for all that there is no defect in our nature, because it is ever the same whether our judgement be true or false'.<sup>2</sup>

The solution of the problem of error which Descartes offers, may be clear and distinct but - to be ironic about his philosophical position - not necessarily correct. He perceives the problem solely from the standpoint of his new epistemological scheme; hence the view that error is characterized by privation of knowledge. However, the correction of an erroneous idea may require not an additional knowledge, but rather the substitution of this idea with another one; Descartes' view is therefore incomplete.

Moreover, he is confused about being, on the one hand, imperfect in using one's freedom, and having, on the other hand, no defect in one's nature since it is constant whatever one's belief is. But when he comes to present some concrete examples of causes of errors, he seems to rehearse Bacon's classification of the idols. In his Principles of Philosophy, Descartes notes examples such as the lasting effects on the understanding of the prejudices of childhood; passing judgements not from present perceptions, but rather from preconceived opinions; and attaching concepts to words which do not accurately answer to the reality.<sup>3</sup>

Furthermore, one cannot properly describe either knowledge or error as wilful: one cannot choose not to err and refuse to know what one thinks

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1. Ibid.

2. Ibid., p.234 (Principles of Philosophy, I, xxxviii).

3. Ibid., pp.249-52 (Principles of Philosophy, I, lxx-lxxiv).

is the case.<sup>1</sup> At all events, the refusal to assent and the prudent wish to be content with a suspension of judgement, may ultimately result in a stagnant situation; from there onwards, the process of attaining knowledge will be at a standstill.

In his rigid deterministic system, Baruch Spinoza (1634-1677) objects categorically to Descartes' concept of wilful assent; 'the will', according to Spinoza, 'cannot be called a free cause, but can only be called necessary'.<sup>2</sup> Moreover, he claims to have demonstrated that 'the will and the intellect are one and the same',<sup>3</sup> and thus to have removed, as he puts it, 'what is commonly thought to be the cause of error'.<sup>4</sup> However, although his system provides a novel setting for the problem of error, Spinoza does not come up with an entirely new way of looking at it.

For Spinoza the human mind is not a free agent that can affirm or deny. He asserts that 'in Nature there is nothing contingent',<sup>5</sup> and holds that 'all things are determined from the necessity of the divine nature to exist and act in a certain manner'.<sup>6</sup> According to Spinoza, the individual mind consists of ideas which are the modifications of that finite mode that bears it, namely, the individual body.<sup>7</sup> These ideas occur therefore in an order which is determined within the order of Nature as a whole.<sup>8</sup> Indeed, as he states, 'the order and connection of ideas is the same as the order and connection of things'.<sup>9</sup>

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1. Evans, 1963, p.139.

2. Spinoza, 1974, p.67 (Pt.I, prop.xxxii).

3. Ibid., p.120 (Pt.II, prop.xlix, corol).

4. Ibid., p.120 (Pt.II, prop.xlix, note). This is a direct reference to Descartes' own view on the origin of errors. Cf., *ibid.*, pp.120-26.

5. Ibid., p.65 (Pt.I, prop.xxix).

6. Ibid.

7. Ibid., p.89 (Pt.II, prop.xiii).

8. Hampshire, 1976, p.107.

9. Spinoza, *op.cit.*, p.83 (Pt.II, prop.vii).

This strict determinism forces Spinoza to deny altogether the possibility of erring, let alone the occurrences of errors. If it were possible to conceive 'a positive mode of thought which shall constitute the form of error or falsity', argues Spinoza, then 'this mode of thought cannot be in God, but outside God it can neither be nor be conceived (for "whatever is, is in God, and nothing can either be or be conceived without God".<sup>1</sup>), and therefore in ideas there is nothing positive on account of which they are called false'.<sup>2</sup> In other words, for Spinoza to conceive an idea - an idea which is related to God, for otherwise it cannot be true nor can it be conceived at all - which renders the concept of error meaningful, amounts to a contradiction.

However, error is a persistent epistemological phenomenon which a system of the most rigorous kind cannot sweep away and decline to acknowledge. Indeed, the problem of error reveals the peculiar difficulty of Spinoza's general position. In his system, he is pledged not to call in the will, nor can he sacrifice the universal validity of thought to account for error. He is therefore bound by his system to find a non-intellectual origin for the ideas which bear no guarantee of truth.<sup>3</sup>

Notwithstanding his rejection of Descartes' concept of the volition of the mind, Spinoza does accept the view that to err is to have relatively incomplete knowledge. In a somewhat similar definition to that of Descartes, Spinoza maintains that 'falsity consists in the privation of knowledge, which inadequate, that is to say, mutilated and confused ideas involve'.<sup>4</sup> In this view the conditions of error are individual bodily images brought together fortuitously. These images do not comply with the order of the

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1. Ibid., p.52 (Pt.I, prop.xv).

2. Ibid., p.107 (Pt.II, prop.xxxiii).

3. Roth, 1924, p.114.

4. Spinoza, op.cit., p.107 (Pt.II, prop.xxxv).

mind; an order which nature as a whole determines. Furthermore, the composition of these bodily images into false ideas is due not to the will of the individual, but rather to the determining influence of environmental factors.<sup>1</sup>

Spinoza locates the non-intellectual origin of error in the imagination, and he expects its elimination as soon as the mind exercises actively the power of thinking over imagining.<sup>2</sup> Spinoza blames therefore the imagination: knowledge from vague experience and from signs, that is, sense-perception and memory, for those mutilated and confused ideas which may occur to the mind.<sup>3</sup> He classifies 'these two ways of looking at things' as knowledge of the first kind, and asserts that it 'alone is the cause of falsity'.<sup>4</sup> He further classifies reason and science as knowledge of the second and third kind respectively, and claims that both are necessarily true.<sup>5</sup> Such a dogmatic doctrine of truth can baffle those who take the view that error may occur in all kinds of knowledge.

Having established these three categories of knowledge - of which the last two provide the ground for distinguishing the true from the false<sup>6</sup> - Spinoza asserts that 'he who has a true idea knows at the same

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1. Roth, op.cit.

2. Ibid., pp.114-16.

3. Spinoza, op.cit., p.112 (Pt.II, prop.xl, note no.2). However, Spinoza concedes that the mind is not necessarily in error because it imagines. 'For', as he explains, 'if the mind, when it imagines non-existent things to be present, could at the same time know that those things did not really exist, it would think its power of imagination to be a virtue of its nature and not a defect'. (Ibid., p.98 (Pt.II, prop. xvii, note).)

4. Ibid., pp.112-13 (Pt.II, prop.xl, note no.2, and prop.xli).

5. Ibid.

6. Ibid., p.113 (Pt.II, prop.xlii).

time that he has a true idea, nor can he doubt the truth of the thing'.<sup>1</sup> However, one of the distinctive features of error is that when one errs, one thinks one knows and thus declines the possibility that one may be wrong. With this view in mind, one may give Spinoza's assertion a twist and observe that the mark of error is that 'he who has an erroneous idea "knows" at the same time that he has a true idea, nor does he intend to doubt the truth of the thing'. Indeed, the posture of the mind, whether one knows or errs, is one and the same; or, as Descartes puts it 'our nature...is ever the same whether our judgement be true or not'.<sup>2</sup>

Since in this view error consists in the privation of knowledge, the implied method of correction is to supplement the error with true ideas. However, as I have pointed out, correcting an error may consist in rejecting its idea rather than in supplementing it with some other ideas. This process,

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1. Ibid., p.113 (Pt.II, prop.xliii). Spinoza seems to suggest that the claim to certainty is an introspective process: 'he who knows a thing truly, must at the same time have an adequate idea or a true knowledge of his knowledge, that is to say', for Spinoza it is self-evident, 'he must be certain'. (Ibid., p.114.) At the bottom of this claim lies the tenet that truth provides its own standard. Metaphorically, Spinoza explains that 'just as light reveals both itself and the darkness, so truth is the standard of itself and of the false'. (Ibid., p.114 (Pt.II, prop.xliii, note).) Spinoza maintains therefore that a true idea 'has no reality nor perfection' over a false one, if the former is distinguished from the latter only in so far as the former is said to agree with that of which it is the idea. Thus, if the distinction is based on external signs alone, then, according to Spinoza, 'the man who has true ideas will have no greater reality or perfection than he who has false ideas only'. (Ibid.) This reflexive concept of truth coupled with the belief in the universal validity of thought, may explain Spinoza's peculiar position with regard to miscalculation. 'When men make errors in calculation', he writes, 'the numbers which are in their minds are not those which are upon the paper. As far as their mind is concerned there is no error, although it seems as if there were because we think that the numbers in their minds are those which are upon the paper. If we did not think so, we should not believe them to be in error'. (Ibid., p.118 (Pt.II, prop.xlvii, note).) Spinoza indeed holds that when people most contradict one another, 'they either think the same things or something different, so that those things which they suppose to be errors and absurdities in another person are not so'. (Ibid.)

2. Supra, p.22 footnote no.2.

although quite common, cannot be sustained in Spinoza's system.<sup>1</sup> Yet Spinoza concludes that he 'removed what is commonly thought to be the cause of error',<sup>2</sup> and 'proved...that falsity consists solely in the privation which mutilated and confused ideas involve'.<sup>3</sup>

In the present panoramic view of the general problem of error, we have seen so far four distinct landmarks. Aristotle acknowledges and analyses the possibility of error in perceiving the common sensibles, and draws attention to the deceptiveness of appearance. He however claims that perception of the specific sensibles is immune to error. Errors, in other words, originate in the transition from sensation to appearance. Bacon starts his new methodology with an analysis of the obstacles to progress, namely, the idols. He identifies four categories of idols which he orders according to their origin: the first type of idols is innate, whereas the fourth is entirely imposed from without. For Bacon errors are prejudices and preconceived ideas which could and should be purged. They have to be eliminated to allow the construction of a true copy of the universe in the human intellect. Descartes examines the mechanism which produces erroneous ideas and finds it to be based on the discrepancy between the scope of the will and that of the understanding. Errors emerge, in his view, when one misuses one's free will and forms judgements on matters which the understanding has not conceived clearly and distinctly. In Spinoza's rigid deterministic view there is no room for what may be called absolute errors, that is, errors which arise out of totally misguided ideas. Errors, according to Spinoza, originate in the privation of knowledge which mutilated and confused ideas involve. They are the consequences of knowledge of the first kind, namely, imagination; however, they will dissolve if they are supplemented with true ideas.

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1. Evans, 1963, pp.140-1.

2. Supra, p. 23 footnote no. 4.

3. Ibid.



Although these views differ, they have in common the belief that knowledge amounts to truth. They all assume that errors can be eliminated, and although they acknowledge fallibility, they do not regard it as a permanent feature of the systems they propound.

John Stuart Mill (1806-1873), who advocates an empirical doctrine, diverges from this dogmatic way. For Mill there is always in natural philosophy some other possible explanation of the same facts: 'some geocentric theory instead of heliocentric, some phlogiston instead of oxygen';<sup>1</sup> in Mill's view, 'it has to be shown why that other theory cannot be the true one: and until this is shown, and until we know how it is shown, we do not understand the grounds of our opinion'.<sup>2</sup> Mill does not pay lip service to fallibility; rather, he considers it almost a principle from which he can derive pluralism - the leitmotiv of his celebrated essay 'On Liberty'.

Mill attributes the sense of complete assurance of the truth of Newtonian physics, to the fact that it has withstood incessant questioning;<sup>3</sup> and he openly admits that 'the beliefs which we have most warrant for have no safeguard to rest on, but a standing invitation to the whole world to prove them unfounded'.<sup>4</sup> Having reached this conclusion, he resigns himself to the view that this is the amount of certainty a fallible being can attain.<sup>5</sup>

However, for Mill the source of everything respectable in man, either as an intellectual or as a moral being, is that his errors are corrigible.<sup>6</sup> In this connection, the moral parallel is quite instructive and worth amplification.

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1. Mill, 1962, p.163.

2. Ibid.

3. Ibid., p.147.

4. Ibid.

5. Ibid.

6. Ibid., p.146.

Consider the religious and moral concept of the Fall of Man. In one of the most glorious hymns from the magnificent liturgy, the Exultet of the Easter Vigil according to the Latin rite, there occurs the seemingly paradoxical phrase 'O felix culpa', that is, 'the Fortunate Fall'. In this religious tradition, Adam's sin has precipitated the latent elements of evil. The question arises therefore as to the way in which the Fall can be fortunate? A possible interpretation suggests that the Fall can be considered a decisive mechanism of purification: through it man can recognize evil and be expected to eject it consciously. In other words, it is Adam's freedom of choice that allowed for his sin and thereby made him bring upon himself an ultimately fortunate fall. It is indeed fortunate, since the recovery from the Fall makes it possible, in effect, to lead a moral life. The Fall has made the awareness of evil possible, and with this knowledge man can choose to struggle back to a moral state, far superior to that which would have been his lot had he never fallen in the first place.<sup>1</sup>

In a similar vein, one may coin the phrase 'O felix erratum', that is, 'the fortunate error'; or, perhaps one should say, 'fortunate errors'. Error, one can maintain, is instrumental in attaining, or, in view of the moral parallel, ascending to, knowledge. As much as the comprehension of the Fall is the signature of moral life, so it is the understanding of error that marks epistemological attainment. 'It is the capacity of making mistakes, not the incapacity of it', as H.H. Price succinctly puts it, 'which is the mark of the higher stages of intelligence.'<sup>2</sup> Indeed Price says, we should congratulate 'any creature which is clever enough to be caught in a trap'.<sup>3</sup>

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1. Werblowsky, 1979, pp.355-62. I am grateful to Y. Bronowski for calling my attention to this paper. Cf., Lovejoy, 1948, pp.277-95.

2. H.H. Price, 1953, p.87.

3. Ibid.

But what are the traps that lurk in the path to knowledge? Mill enquires into this problem and attempts to elucidate the nature of these traps. He prefaces his book on fallacies - the fifth in his System of Logic - with Malebranche's view that it is not enough to say in general that the human nature is infirm, and just to be on guard against one's prejudices. To be content with the admission that the spirit is subject to error, would not suffice; one has to make the spirit realize in what its error consists.<sup>1</sup> And indeed Mill opens the book with the maxim that 'we never really know what a thing is, unless we are also able to give a sufficient account of its opposite'.<sup>2</sup>

Although Mill advocates an empirical doctrine, he nevertheless addresses himself explicitly to the problem of fallacy - his study is a classification of fallacies - and not to that of error, let alone experimental error. This attitude does not indicate in itself that Mill evades the issue of error in science altogether. On the contrary, upon analysing, for example, chance and its elimination, he observes that unavoidable errors can affect the result of an individual experiment.<sup>3</sup> He states, quite in accordance with the mathematical theory of error, that 'we have...to repeat the experiment, until any change which is produced in the average of the whole by further repetition, falls within the limits of error consistent with the degree of accuracy required by the purpose we have in view'.<sup>4</sup> Moreover, looking for evidence of universal causation, he admits that 'errors... may have slipped into the statement of any one of the special laws, through inattention to some material circumstances; and instead of the true

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1. Mill, 1843, p.332.

2. Ibid., p.333.

3. Ibid., p.65.

4. Ibid., p.66.

proposition, another may have been enunciated, false as an universal law'.<sup>1</sup> And noting that observation is the first stage among the operations subsidiary to induction, he suggests that one has to consider under what conditions observation is to be relied on;<sup>2</sup> 'what is needful', as he puts it, 'in order that the fact, supposed to be observed, may safely be received as true'.<sup>3</sup> Mill, in this instance, follows Aristotle and claims that sensations are always genuine. Errors of the senses, according to Mill, are in fact erroneous inferences from the senses.<sup>4</sup>

Mill seems therefore to be quite aware of the general problem of error and, as an empiricist, of the problem of experimental error which arises in experiments. Thus, notwithstanding the title of Mill's study, namely, classification of fallacies, which pertains to logic, one who is interested in the problem of error will look forward to examining this classification; the disappointment, however, is considerable.

Mill distinguishes five classes of fallacy, which he expresses in the following synoptic table:<sup>5</sup>

Fallacies	{	of Simple Inspection	-	-	1. Fallacies <i>à priori</i> .
					2. Fallacies of Observation.
	{	from evidence distinctly conceived	{	Inductive Fallacies	3. Fallacies of Generalization.
		from evidence indistinctly conceived	{	Deductive Fallacies	4. Fallacies of Ratiocination.
		(of Inference)		- - -	5. Fallacies of Confusion.

1. Ibid., p.118. However, Mill holds that such errors cannot undermine the general law of causation. Indeed, he states that the law 'would remain unaffected by any such error' (Ibid., p.119). For Mill, 'the law of cause and effect is therefore... placed, in point of certainty, at the head of all our induction'. (Ibid.)
2. Ibid., pp.201-2.
3. Ibid., p.202.
4. 'Innumerable instances might be given, and analysed', Mill writes 'of what are vulgarly called errors of sense. There are none of them properly errors of sense; they are erroneous inferences from sense.' Mill explains that 'the deception, whether durable or only momentary, is in my judgement. From my senses I have only the sensations, and those are genuine'. (Ibid., pp.202-4.)
5. Ibid., p.344.

Fallacies a priori are false beliefs, prejudices or superstitions which bias the observer upon examining a subject matter. These fallacies are of simple inspection and Mill distinguishes them from the rest of the four categories which are of inference. Thus, the four remaining categories are concerned with erroneous conclusions from supposed evidence, and are divided, therefore, according to the nature of the supposed evidence from which the conclusions are drawn.<sup>1</sup>

The first two categories of inference pertain to the inductive method. Fallacies of observation constitute the class of all inductions of which, as Mill puts it, 'the error lies in not sufficiently ascertaining the facts on which the theory is grounded'.<sup>2</sup> The second category of the inductive method is that of faulty induction and false analogies, that is, the facts are correct but they do not warrant the conclusion.<sup>3</sup> The deductive fallacies, those of ratiocination, are clearly formal, namely, modes of incorrect argumentation; they consist mainly of vicious syllogism.<sup>4</sup> The final category represents a miscellaneous collection of fallacies in which the source of error is an indistinct, indefinite, and fluctuating conception of what the evidence is, such as question begging and irrelevant conclusion.<sup>5</sup>

The interesting thing about this classification is that though it bears the mark of Mill's empiricism, namely, it includes the method of induction, it still retains the features of a classification of logical fallacies - the emphasis being on the descriptive term 'logical'. However, it seems that Mill does not strike the right balance between fallacy and error. The interchangeable use which he makes of the terms 'fallacy',

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1. Ibid., pp.340-1.

2. Ibid., p.343.

3. Ibid.

4. Ibid., pp.343-4.

5. Ibid., pp.342, 344.

'error' and 'mistake', suggests that he is much more concerned with logic than with the method of experimentation. Appropriately to a system of logic, Mill stresses the logical aspect of reasoning, namely, inferences, but that is achieved at the expense of an account of errors in general and experimental errors in particular. Notice that although Mill professes an empirical doctrine, his only category of experimentation, namely, the category of fallacies of observation, is subsumed under fallacies of inference. Moreover, in this category he points out that induction is not always grounded upon facts immediately observed; one should also consider the case of facts inferred. When the latter are erroneous, the error is not, as Mill explains, 'an instance of bad observation, but of bad inference'.<sup>1</sup> He thus draws a distinction between what he calls mal-observation and simple non-observation.<sup>2</sup> In his view, mal-observation may occur due to mistaking inferences for perception; for 'the logic of observation', as Mill holds, 'consists solely in a correct discrimination between that... which has really been perceived, and that which is an inference from the perception'.<sup>3</sup>

Evidently, the underlying criterion of Mill's classification is logical; his professed empiricism seems to bear layers of the rationalistic outlook. Nevertheless, Mill's attempt to classify experimental errors - albeit only observational errors - within a classification of fallacies, points at the right direction for carrying out the enterprise of classifying experimental errors in a comprehensive fashion.

The positive feature of error - its role, that is, in the process of attaining knowledge - has been captured by Karl R. Popper who calls his philosophical position 'critical rationalism'.<sup>4</sup> Critical rationalism consists, according to Popper, in replacing the standard epistemological

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1. Ibid., p.343.

2. Ibid.

3. Ibid., p.205.

4. Popper, 1974, p.26.

problem as to the sources of our knowledge, with the entirely different question of 'how can we hope to detect and eliminate error?'<sup>1</sup> Popper derives this approach from the belief that untainted, certain and pure sources of knowledge do not exist, and from the demand that questions of origin or of purity should not be confounded with questions of validity, or of truth.<sup>2</sup> As the name 'critical rationalism' suggests, the solution that Popper offers is dependent upon the application of our critical faculty. It is by criticizing theories or guesses, Popper maintains, that one may hope to detect and eliminate error; by criticizing Popper means, as is well known, the attempts to refute, or to provide counter examples for, the proposed conjectures.<sup>3</sup> Error thus becomes, in the hands of Popper, a methodological tool, an essential tool in carrying out the falsification programme.

Though Popper is right in stressing the importance of error as a methodological tool, he seems to ignore, nevertheless, its possible ontological implications. This indifference is largely due to his philosophical stand; in a different philosophical framework, from the standpoint of physical realism for example, these other aspects of error do come to the fore. I shall indeed argue<sup>4</sup> that it was the belief in physical realism which made it possible for Johannes Kepler (1571-1630) to attain a comprehensive view of the concept of error.

Kepler's mode of thought is not that of the rationalist;<sup>5</sup> error for Kepler is not just a methodological tool that can, to use his own words, 'show us the way to truth'.<sup>6</sup> It also reflects - albeit negatively - the

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1. Ibid., pp.25-6.

2. Ibid., p.25.

3. Ibid., p.26.

4. *Infra*, Ch.III.

5. See, e.g., Pauli, 1955, p.171.

6. Quoted by Koyré, 1973, p.250.

existence of a physically real world. Being much influenced by the theological concept of the triune Godhead - to which he remained faithful throughout his life - Kepler had commenced his study of planetary motions with the principle of circular motion at its basis. As he was able to respond positively to actual measurements, he later on realized that this principle had led him astray. Consequently, he corrected and re-directed his research to an eventual success.<sup>1</sup>

The negative reflection of the physical world through the occurrence of errors, reminds one of St. Augustine's (354-430) dictum: 'If I am mistaken, I exist.' St. Augustine claims that 'a non-existent being cannot be mistaken; therefore', he argues, 'I must exist, if I am mistaken';<sup>2</sup> a far cry from Descartes' maxim: 'cogito ergo sum'.

In St. Augustine's view one cannot go wrong except through ignorance;<sup>3</sup> however, ignorance can be either avoidable or unavoidable. One may misinterpret a text because one's knowledge of its language is faulty, but it can also be misinterpreted because certain circumstances about it were not known to its interpreter.<sup>4</sup>

Throughout the thesis I try to sustain this distinction between the two ways of going wrong which I call respectively the way of mistake and the way of error. I associate mistake with avoidable ignorance on the basis of the availability of checking procedures, and contrast it with error which springs from unavoidable ignorance when one is groping, so to speak, in the dark. Metaphorically, a mistake occurs when one goes wrong on terra firma; but getting lost in one's exploration of terra incognita amounts to an error.

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1. Pauli, op.cit. I elaborate this point in Ch.III.

2. St. Augustine, 1972, p.460 (Bk.XI, Ch.26).

3. St. Augustine, 1961, p.18 (xvii).

4. Momigliano, 1975, p.368.



In this sense, a printer's error - to return to Bacon's analogy - is really a mistake, whereas an experimental error is indeed an error though it may contain as a general concept elements of mistake such as miscalculation. A printer's error can never be an error in the sense that I am here propounding; it does not arise out of an unavoidable ignorance, and there is indeed an available agreed procedure to rectify it, namely, proof-reading.

Although in both the concept of mistake and the concept of error one essentially takes the false for the true, and the true for the false, the two concepts carry, as it were, different epistemological weight. As I shall explain presently, it is the concept of error which is epistemologically of more importance and indeed interest.

In order to maintain the distinction between the true and the false - a distinction which is essential for both the concept of mistake and the concept of error to be significant - there must be a certain criterion which allows one to uphold it. In other words, to use Michael Dummett's formulation, 'for any statement which has a definite sense, there must be something in virtue of which either it or its negation is true'.<sup>1</sup> The <sup>one</sup> fact that/can assign the truth-value 'true' to a definite statement shows that there is something in virtue of which the statement is true. As Dummett further explains, the meaning of the claim that 'there is something in virtue of which the statement is true', amounts to that: that 'there is something such that if we knew of it we should regard it as a criterion (or at least as a ground) for asserting the statement'.<sup>2</sup> It is the existence of such a criterion, such a ground - which allows one to assign either the truth-value 'true' or 'false' to a statement - that creates the condition for making a mistake, for committing an error.

The view that there exists something in virtue of which a definite

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1. Dummett, 1970, p.433.

2. Ibid.

statement can be judged to be either true or false, represents in a nutshell the doctrine of realism.<sup>1</sup> I suggest to call the criterion, or the ground, which allows for this judgement, element of reality.

Following up the distinction between mistake and error, we may note that in the case of a mistake elements of reality amount to rules. The existence of a rule - in particular, a rule of computation or one governing the use of a word or symbol - rests ultimately upon the fact that there is an agreement in practice over its application.<sup>2</sup> A rule may be misapplied for a variety of reasons; but in so far as we can agree in assigning the truth-value 'true' to a proposition governed by a rule, we can detect a misapplication, that is, a mistake, in every procedure which does not, under the same circumstances, produce the same proposition. A straightforward example is miscalculation. It seems that with respect to error immediate and rigorous methods of detection are not available or even not known. Indeed, it is precisely because one is lacking a knowledge of those elements of reality which make a statement erroneous, that error becomes epistemologically interesting. An error of interpretation may serve as an example; an erroneous interpretation of experimental data may withstand all kinds of criticism and re-examination. Only in the light of some other results - theoretical or experimental - might it be shown that the interpretation is in error: the relevant elements of reality being understood. In sum, there exist, on the operative level, agreed procedures by which a mistake can be always, in principle, identified and rectified; whereas the procedures of countering an error are left entirely - in the domain of experimentation - to the ingenuity of the experimenter or his critics.

In Kepler's hands elements of reality have come to represent the concrete physical world, and it is with this view in mind that I have singled out his work. That is, my interest in Kepler lies not so much in the

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1. Ibid.

2. Dummett, 1978, p.65.

historical figure, as in his philosophical make-up which allows for a comprehensive approach towards the problem of error.

An interesting transition from one type of elements of reality to another occurs in Wittgenstein's philosophy. Such a transition forces the issue of elements of reality to the open, and thus forms a fertile ground for discussing <sup>the</sup> problem of going wrong: of making mistakes and committing errors. In his Notebooks (15.10.1916), Ludwig Wittgenstein (1889-1951) reports on his philosophical progress as follows:

This is the way I have travelled: Idealism singles out men from the world as unique, solipsism singles me alone out, and at last I see that I too belong with the rest of the world, and so on one side nothing is left over, and on the other side, as unique, the world.<sup>1</sup> In this way, idealism leads to realism if it is strictly thought out.

This philosophical progress has been distilled in the Tractatus - Wittgenstein's philosophical debut - where he states that

solipsism, when its implications are followed out strictly, coincides with pure realism. The self of solipsism shrinks to a point without extension, and there remains the reality co-ordinated with it.<sup>2</sup>

Indeed, realism provides the backbone of the Tractatus: 'A proposition is a picture of reality';<sup>3</sup> 'Reality is compared with propositions';<sup>4</sup> and hence, 'A proposition can be true or false only in virtue of being a picture of reality'.<sup>5</sup>

However, in On Certainty - a collection of Wittgenstein's last notes - the schematic view of pure realism broadens its horizon to include the mature view of the later Wittgenstein. Here the claim to realism relies on the inherited background. 'I did not get my picture of the world by satisfying myself of its correctness,' he states, 'nor do I have it because

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1. Vesey, 1974, pp.98-99.

2. Wittgenstein, 1978, p.58 (§5.64).

3. Ibid., p.19 (§4.01).

4. Ibid., p.23 (§4.05)

5. Ibid., p.23 (§4.06).

I am satisfied of its correctness. No: it is the inherited background against which I distinguish between true and false.'<sup>1</sup> At another juncture of this collection of remarks – where Wittgenstein argues against regarding an experimental result a proof of a certain proposition<sup>2</sup> – he explains that the expression "we are quite sure of it" does not mean just that every single person is certain of it, but that we belong to a community which is bound together by science and education'.<sup>3</sup>

Although these two works profess realism in so far as they both grapple with the problem of assigning truth-values to definite propositions, the elements of reality which they consider differ greatly. Whereas in the Tractatus an element of reality regulates a direct correspondence between the so-called pure reality and the proposition which claims to depict it, in On Certainty such an element mediates between the two via a cultural context.

Concerning the problem of mistake and error, On Certainty is rather more revealing than the Tractatus, and contains some observations which can throw light on the difficulties this thesis attempts to tackle. Indeed, the issues which On Certainty raises, form the epistemological background of the problem of mistake and error. For, after all, a proposition can be said to be certain only if it is inconceivable that one should be wrong in thinking it true.<sup>4</sup> In view of the inherent fallibility of human nature, the question then arises as to the validity of the certainty which mathematical, let alone empirical, propositions claim.

Examining the claim to certainty, Wittgenstein remarks that 'with the word "certain" we express complete conviction, the total absence of

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1. Wittgenstein, 1977, p.15e (§94).

2. Ibid., p.38e (§§ 292-97).

3. Ibid., p.38e (§298). Hence his remarks on authority: 'So is this it: I must recognize certain authorities in order to make judgements at all?' (Ibid., p.65e (§493).) See also p.23e (§161).

4. Ayer, 1974, p.232.

doubt, and thereby we seek to convince other people'.<sup>1</sup> However, in Wittgenstein's view, this is only subjective certainty; he thus presses on to ask 'when is something objectively certain?'<sup>2</sup> That kind of certainty may be attained when, according to Wittgenstein, a mistake is entirely impossible.<sup>3</sup> The claim that one cannot be making a mistake about a certain proposition, is commonly used; 'but we may question', Wittgenstein writes, 'whether it is then to be taken in a perfectly rigorous sense, or is rather a kind of exaggeration which perhaps is used only with a view to persuasion.'<sup>4</sup>

The problem as to what is precisely the nature of the claim that one cannot be making a mistake about a certain proposition, features prominently in On Certainty. Wittgenstein hinges this problem on the principal issue of assigning the truth-value 'true' or 'false'; here he sees the core of the problem, for in his view the expression 'true or false' has something misleading about it. As he puts it,

it is like saying "it tallies with the facts or it doesn't", and the very thing that is in question is what "tallying" is here. Really "the proposition is either true or false" only means that it must be possible to decide for or against it.<sup>5</sup> But this does not say what the ground for such a decision is like.<sup>5</sup>

Moreover, that ground has to be established objectively so that objective certainty will be attained. In Wittgenstein's words,

it needs to be shewn that no mistake was possible. Giving the assurance "I know" does not suffice. For it is after all only an assurance that I can't be making a mistake, and it needs to be objectively established that I am not making a mistake about that... "I know that" means "I am incapable of being wrong about that". But whether I am so must admit of being established objectively.<sup>6</sup>

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1. Wittgenstein, 1977, p.27e (§194).

2. Ibid.

3. Ibid.

4. Ibid., p.88e (§669); cf., ibid., (§668).

5. Ibid., p.27e (§§199-200).

6. Ibid., p.4e (§§15-16). 'I know that p' is in place only where I can give grounds for p that are surer than p; in practice, this is generally not the case. (Ibid., p.32 e (§243).) This is part of Wittgenstein's criticism of Moore's proof of the external world. Cf., Kenny, 1976, Ch.11. Kolakowski remarks that 'the predicates "true" and "false" are not found in experience... They belong to the human interpretation of experience.' (Kolakowski, 1975, pp.15-16.)

The claim 'I can't be making a mistake about that', has obviously intrigued Wittgenstein. 'What if it does turn out to be wrong?'<sup>1</sup> he asks. One can of course make this claim, but some day, rightly or wrongly, one may think one realizes that one was not competent to judge.<sup>2</sup> And then, Wittgenstein enquires, 'mustn't one make a distinction between the ways in which something "turns out wrong"? - How can it be shewn that my statement was wrong? Here evidence is facing evidence, and it must be decided which is to give way'.<sup>3</sup>

To illustrate this difficulty, Wittgenstein examines a proposition from day to day physics.

We say we know that water boils and does not freeze under such-and-such circumstances. Is it conceivable that we are wrong? Wouldn't a mistake topple all judgement with it? More: what could stand if that were to fall? Might someone discover something that made us say "It was a mistake"? Whatever may happen in the future, however water may behave in the future, - we know that up to now it has behaved thus in innumerable instances.<sup>4</sup>

The purpose of the phrase 'I know', or rather 'we know', appears to indicate, in this instance, a reliance which in Wittgenstein's view must emerge from experience.<sup>5</sup> But then, are we not trapped in a circular argument?

Wittgenstein seems to hint here at the impossibility - at least with regard to a large class of empirical propositions - of establishing objectively that one cannot go wrong. Although he regards the claims these propositions make as knowledge, he nevertheless implies that this knowledge is temporal.<sup>6</sup> By making an experiment a few times, one discovers that under such circumstances this happens; in the final analysis, one relies,

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1. Wittgenstein, *ibid.*, p.84e (§641).

2. *Ibid.*, p.85e (§645).

3. *Ibid.*, p.84e-85e (§641).

4. *Ibid.*, p.73e (§558).

5. *Ibid.*, p.75e (§575).

6. I use the term 'temporal' so that it will stand in contrast to the claims of propositions of grammar which Wittgenstein characterizes as 'not temporal'. (*Ibid.*, p.9e (§57).)

according to Wittgenstein, upon such an experience or its report. 'But hasn't this trust also proved itself?' Wittgenstein asks and proceeds to answer that 'so far as I can judge - yes'.<sup>1</sup> In other words, the presence of the possibility of doubt, and even mistake, does not undermine the possibility of knowledge. Indeed, Wittgenstein seems to suggest that the concept of knowledge can arise only where there is such a thing as being wrong;<sup>2</sup> a view which echoes Aristotle's complementary approach to the concepts of knowledge and error. However, there still seem to be types of proposition which can be rendered certain on objective grounds, namely, a priori propositions of logic and mathematics. Yet the problem of being wrong persists even here; evidently, it is possible to make mistakes in logic and mathematics, and one cannot always be sure of the conclusions to which they appear to lead.

In his study of the nature of proposition and its relation to reality, Wittgenstein returns again and again to the question as to what precisely is the difference between a mathematical proposition and an empirical one; that is, the difference for example between a proposition which states a calculation and one which represents an experimental result. Dummett, in his paper, 'Wittgenstein's Philosophy of Mathematics', sets the scene for this problem in a very clear fashion.

A mathematical proof, of which computations are a special case, is a proof in virtue of our using it to serve a certain purpose; namely, we put the conclusion or result in the archives, that is, treat it as unassailable and use it as a standard whereby to judge other results. Now something cannot serve this purpose, and hence is not a mathematical proof, unless we are able to exclude the possibility of a mistake's having occurred in it. We must be able to "take in" a proof, and this means that we must be certain of being able to produce the same proof. We cannot in general guarantee that we shall be able to repeat an experiment and get the same result as before. Admittedly, if we get a different result, we shall look for a relevant difference in the conditions of the experiment; but we did not have in advance a clear conception of just what was to count as a relevant difference.<sup>3</sup>

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1. Ibid., pp.79e-80e (§603).

2. See Coope, 1974, p.258.

3. Dummett, 1970, p.440.

Dummett omits the possible statistical spread of an experimental result, but he is amply clear about the possibility, indeed the requirement, of eliminating all the mistakes which may occur in a mathematical proof; as Wittgenstein puts it: 'Mustn't mistakes be logically excluded?'<sup>1</sup> But then, Wittgenstein asks, 'can it be seen from a rule what circumstances logically exclude a mistake in the employment of rules of calculation? What use is a rule to us here? Mightn't we (in turn) go wrong in applying it?'<sup>2</sup> Wittgenstein ponders on this issue but does not arrive at a definite conclusion. He thinks that, on the one hand, one 'cannot be making a mistake about  $12 \times 12$  being 144';<sup>3</sup> but, on the other hand, a mathematical proposition has been obtained by a series of actions which, as he observes, 'are in no way different from the actions of the rest of our lives, and are in the same degree liable to forgetfulness, oversight and illusion'.<sup>4</sup> Wittgenstein claims therefore that 'if the proposition  $12 \times 12 = 144$  is exempt from doubt, then so too must non-mathematical propositions be'.<sup>5</sup> Moreover, in his view, 'the same proposition may get treated at one time as something to test by experience, at another as a rule of testing'.<sup>6</sup> He thus makes the proposition depend on the context of its use, notwithstanding his awareness of the 'official' stamp of incontestability which mathematical propositions have been given.<sup>7</sup> It is only with respect to logic that Wittgenstein feels sufficiently determined to state that it would be wrong to claim that logic too can be an empirical science.<sup>8</sup>

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1. Wittgenstein, 1977, p.27e (§194).

2. Ibid., p.5e (§26).

3. Ibid., p.86e (§651).

4. Ibid. 'The question "But mightn't you be in the grip of a delusion now and perhaps later find this out?" - might also be raised as an objection to any proposition of the multiplication tables.' (Ibid., p.87e (§658).)

5. Ibid., p.86e (§653).

6. Ibid., p.15e (§98). Cf. p.24e (§163) and p.41e (§321).

7. Ibid., p.87e (§655).

8. Ibid., p.15e (§98).



Although Wittgenstein suggests that distinctions between the ways in which something 'turns out wrong' should be made, he does not differentiate between mistake and error as two different modes of being wrong, and rather uses the two terms interchangeably.<sup>1</sup> To be sure, Wittgenstein is intent on making distinctions; indeed a dominant methodological idea of his philosophy is to search for dissimilarities, especially among things which appear similar, in an attempt to resolve philosophical confusions. In On Certainty he studies, as we have seen, the nature of propositions from the point of view of their failure to be true and attain certainty, and seeks distinctions from this perspective. He contrasts the empirical proposition 'At this distance from the sun there is a planet' with another empirical proposition, viz. 'Here is a hand' (namely, Wittgenstein's hand), and admits that 'there isn't a sharp boundary line between them'.<sup>2</sup> However, having examined these two propositions from the point of view of the question as to 'what could a mistake here be like?'<sup>3</sup> he locates the two at the extremes of, as it were, a spectrum of types of empirical proposition, and claims that 'it is not true that a mistake merely gets more and more improbable as we pass from the planet to my own hand. No: at some point it has ceased to be conceivable'.<sup>4</sup> And correspondingly, 'doubt', as he puts it, 'gradually loses its sense'.<sup>5</sup> In Wittgenstein's view 'there are countless general empirical propositions that count as certain for us'.<sup>6</sup> For, otherwise,

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1. See, e.g., *ibid.*, p.86e (§650) and the German text.
  2. *Ibid.*, p.9e (§52).
  3. *Ibid.*, p.9e (§51).
  4. *Ibid.*, p.9e (§54).
  5. *Ibid.*, p.9e (§56). However, Wittgenstein, unlike Descartes, is aware of the false implication that in cases where one could not doubt, one could not be mistaken either. Cf., Kenny, 1976, pp.207-8.
  6. Wittgenstein, *ibid.*, p.35e (§273). 'We teach a child "that is your hand", not "that is perhaps (or "probably") your hand"... An investigation or question, 'whether this is really a hand' never occurs to him.' (*Ibid.*, p.48e (§374).) 'But how is it for example with anatomy (or a large part of it)? Isn't what it describes, too, exempt from all doubt?' (*Ibid.*, p.88e (§666).)

he explains, 'it would also be conceivable that we should be wrong in every statement about physical objects; that any we ever make are mistaken'.<sup>1</sup>

Wittgenstein's analysis has led him to put empirical propositions such as 'This is my hand' on a par with propositions of arithmetic (e.g., the multiplication tables).<sup>2</sup> The latter propositions have the character of a rule; they cannot be doubted, as Wittgenstein explains, without giving up all judgement.<sup>3</sup> However, he equally realizes that no doubt can exist about certain empirical propositions if making judgements is to be possible at all. 'I am inclined to believe,' he writes, 'that not everything that has the form of an empirical proposition is one.'<sup>4</sup> He admits that 'one may be wrong even about "there being a hand here". Only in particular circumstances is it impossible;' but then, as he remarks, 'even in a calculation one can be wrong - only in certain circumstances one can't'.<sup>5</sup>

Wittgenstein propounds therefore the view that there is no sharp boundary either between the a priori propositions of logic, mathematics and, one may add, grammar, and some propositions that would ordinarily be counted as empirical, or between these propositions and those of which the empirical character is not at all in question.<sup>6</sup> Having blurred the sharp line of demarcation which is commonly held between a priori propositions of logic and mathematics, and those which contain empirical content, he asks: 'Is it that rule and empirical proposition merge into one another?'<sup>7</sup> And indeed he maintains that it is due to the fuzzy boundary

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1. Ibid., p.9e (§54).

2. 'I want to say: If one doesn't marvel at the fact that the propositions of arithmetic (e.g. the multiplication tables) are 'absolutely certain', then why should one be astonished that the proposition "This is my hand" is so equally?' (Ibid., p.58e (§448).) Cf., ibid., p.87e (§657).

3. See, e.g., ibid., p.65e (§494).

4. Ibid., p.39e (§308).

5. Ibid., p.5e (§25). Cf., ibid., p.29e (§217).

6. Ayer, 1974, p.234.

7. Wittgenstein, 1977, p.39e (§309).

between rule and empirical proposition that the demarcation between the different types of proposition is not sharp.<sup>1</sup>

However, there is still the possibility of distinguishing between the ways in which something 'turns out wrong'; that is, the differentiation between different modes of being wrong. Wittgenstein acknowledges that 'not all corrections of our views are on the same level';<sup>2</sup> indeed he writes that he distinguished between different kinds of mistake.<sup>3</sup> However, he does not seem to have carried this programme through. The closest he gets to it is to distinguish mistake from mental disturbance, however transient the latter may be.<sup>4</sup> 'Can we say,' he asks rhetorically, that 'a mistake doesn't only have a cause, it also has a ground?' In other words, as he explains, 'when someone makes a mistake, this can be fitted into what he knows aright.'<sup>5</sup>

This characterization matches well with my suggestion of distinguishing between mistake and error. It is not that error, in contrast to mistake, is lacking a ground; it is rather that a ground, in the case of error, is merely not known. Thus, when one commits an error, to paraphrase Wittgenstein, this cannot be fitted into what he knows aright; for in this case the relevant elements of reality, to recall my terminology, are not known. However, in the case of mental disturbance a ground is entirely missing; and, in accordance with this distinction, a mistake reflects the existence of a rule that could and should be known aright. As Wittgenstein

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1. 'But wouldn't one have to say then, that there is no sharp boundary between propositions of logic and empirical propositions? The lack of sharpness is that of the boundary between rule and empirical proposition.' (Ibid., p.41e (§319).)
  2. Ibid., p.38e (§300).
  3. Ibid., p.87e (§659).
  4. Ibid., p.11e (§71).
  5. Ibid., p.11e (§74). Cf., ibid., p.85e (§647). On the distinction between mistake and mental disturbance see Kenny, 1976, pp.207-211.

writes, 'in order to make a mistake, a man must already judge in conformity with mankind'.<sup>1</sup> In my view, this distinction between mistake and error can sharpen the demarcation line between propositions whose truth-values depend on rules and their application, and those which involve in the assignment of their truth-values elements of reality other than rules.

Be that as it may, the intricate attempt to distinguish between different types of proposition from the perspective of their valid, or rather invalid, claims to certainty, seems to have motivated Wittgenstein to underline what I take to be two major themes of the problem of error in its general sense. The first theme stresses the difficulty of establishing objectively that one is not wrong. The second theme suggests - in view of the fact that there are after all different types of proposition - to distinguish between the different ways in which something 'turns out wrong'. In my thesis I elaborate the latter theme and acknowledge the former; to put it concisely, this thesis dwells upon the different ways in which an experiment may turn out wrong.

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1. Wittgenstein, 1977, p.23e (§156).

## CHAPTER II

### On the Concept of Experimental Error in Greek Science

The attempt to narrow the general discourse of the problem of error and to focus it on the specific problem of experimental error may be approached from different directions. One possibility is to establish a focusing process from the standpoint of history; such an approach requires a careful scrutiny of the history of science with a view to identifying the juncture when the problem was properly understood and addressed. In a study of this kind one would have to examine the evolution of the concept of experiment and related topics so that clear criteria would underlie the analysis.

However, this is not what I propose to do, if only for the reason that an historical analysis would require a thesis of its own. Rather, I propose to bring the problem of experimental error to the fore by contrasting two different attitudes towards this problem. I shall address myself principally to the question as to why it was not permissible for Kepler to ignore a discrepancy of 8 minutes of arc; 8 minutes which, in Kepler's words, 'led the way to the reformation of the whole of astronomy'.<sup>1</sup> In other words, it is the perception that one is in error which I propose to pursue and analyse. In contrast to this awareness, I shall discuss briefly a few suggestive cases from Greek astronomy in which such an awareness is at best only implicit or indeed lacking altogether. I shall attempt further to set this contrast against a philosophical background so that the essential philosophical elements which are conducive to the understanding of the problem of experimental error will be at hand. It is thus within the framework of the history of ideas that my focusing process lies.

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1. Kepler's words are: 'Nunc quia contemni non potuerunt, sola igitur haec octo minutaviam praeiverunt ad totam Astronomiam reformatandam, suntque materia magnae parti hujus operis facta.' Quoted by Koyré, 1973, p.401, note no.22.

In a famous passage in the Republic, Plato (428-348 B.C.) expresses a view which *prima facie* amounts to a categorical objection to the observational method, and by implication to the method of experimentation. Notwithstanding his acceptance of the view that 'the stars that decorate the sky... [are] the finest and most perfect of visible things', they are, Plato maintains, 'far inferior, just because they are visible, to the pure realities'.<sup>1</sup> For Plato it is the true relative velocities, in pure numbers and perfect figures, of the planets and their orbits, which constitute realities; and these are perceptible, in his view, to reason and thought but not visible to the eye.<sup>2</sup> He therefore argues that 'if anyone tries to learn anything about the world of sense whether by gaping upwards or blinking downwards, I don't reckon that he really learns - there is no knowledge to be had of such things'.<sup>3</sup> Astronomy should be treated, according to Plato, like geometry, that is, a discipline which sets problems for solution.<sup>4</sup> Thus, in order to make a genuine study of this subject, one should ignore the visible heavens.<sup>5</sup> Plato indeed applies this principle further and comments that the Pythagoreans are 'wasting their time on measuring audible concords and notes against each other'.<sup>6</sup> He does not think much of these people who 'torment the strings and try to wring the truth out of them by twisting them on pegs'.<sup>7</sup> He seems to despise the attempt to 'look for numerical relationships in audible concords'.<sup>8</sup> In sum, concerning both astronomy and harmonics, Plato appears, in these passages of the Republic, to object to the preference of the senses over mind.

However, as F.M.Cornford points out, 'Plato's primary purpose here

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1. Plato, 1974, p.338 (529d).

2. Ibid.

3. Ibid., (529b-c).

4. Ibid., p.339 (530b).

5. Ibid., (530b-c).

6. Ibid., p.340 (531).

7. Ibid., (531b).

8. Ibid., p.342 (531c).

is not to advance physical science, but to train the mind to think abstractly'.<sup>1</sup> In this sense, astronomy should be considered a study which can make the Guardians cultivate reason rather than the senses.<sup>2</sup> Nevertheless, Plato's didactic injunction to ignore the visible heavens was taken out of context in antiquity, as it has been again in modern times, to be construed as a ban on observational methods as a whole.<sup>3</sup> It appears that this doctrine of Plato has had great influence upon the interpretation of Greek sciences.

'But if modern scholars had devoted as much attention to Galen or Ptolemy as they did to Plato and his followers, they would have come,' O. Neugebauer argues, 'to quite different results and they would not have invented the myth about the remarkable quality of the so-called Greek mind to develop scientific theories without resorting to experimental or empirical tests.'<sup>4</sup>

In Neugebauer's view,

it is not because of philosophical prejudices that the Ptolemaic system dominated astronomy for about 1500 years but because of the solidity of its empirical foundations.<sup>5</sup>

Indeed, as G.E.R. Lloyd has convincingly demonstrated,<sup>6</sup> the notion that Plato directed, so to speak, Greek sciences away from empirical grounds, cannot be sustained by a careful study. It is by now an established historical fact that Greek sciences include many observational results - obtained either directly or through experimentation - which were incorporated into theories. However, a crucial question arises as to the way observational results were incorporated: whether they were considered a critical means of testing theories, or a mere corroborative device for the purpose of persuasion - a device over which theories can take precedence?

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1. Cornford, 1966, p.241.

2. Lloyd, 1979, p.132.

3. Ibid., p.133.

4. Neugebauer, 1969, p.152; see also p.69.

5. Neugebauer, 1956, p.296. Quoted by Palter, 1970, p.127 note no.3. However, see *infra*, p.60 note no.5.

6. Lloyd, 1979.

From the point of view of the problem of experimental error, this question is all the more important since its answer can afford a clue to the understanding of the attitude the Greeks held towards the problem of experimental error. I shall defer the discussion of this question and presently delineate a few cases which can provide a ground, however sketchy, for such a discussion.

According to Neugebauer, Aristarchus (310-230 B.C.) can be considered the first astronomer who demonstrated that out of a few observational data combined with purely mathematical arguments, one could glean information about the sizes of the moon, the sun and their distances from earth. Aristarchus thus established a new methodological principle which is based on empirical and rational arguments.<sup>1</sup> Yet, much of his astronomy shows, as Neugebauer puts it, 'a lack of interest in empirical numerical data in contrast to the emphasis on the purely mathematical structure'.<sup>2</sup>

In his only preserved treatise, On the Sizes and Distances of the Sun and Moon,<sup>3</sup> Aristarchus deduced the result that the distance of the sun from earth is between eighteen and twenty times as great as that of the moon from earth. This result, which *prima facie* indicates a certain awareness of what a physical measurement consists of - in that it gives upper and lower bounds - was arrived at through correct geometrical, and therefore theoretical, considerations but on the basis of impracticable observational method which as a consequence involved incorrect magnitudes of astronomical parameters.

Furthermore, the ratios of the sizes and distances which Aristarchus set himself to calculate are, according to his geometrical construction, trigonometrical. In Aristarchus' time neither had such ratios been calculated, nor had a reasonably close approximation to the value of  $\pi$  been

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1. Neugebauer, 1975, p.659.

3. Heath, 1913.

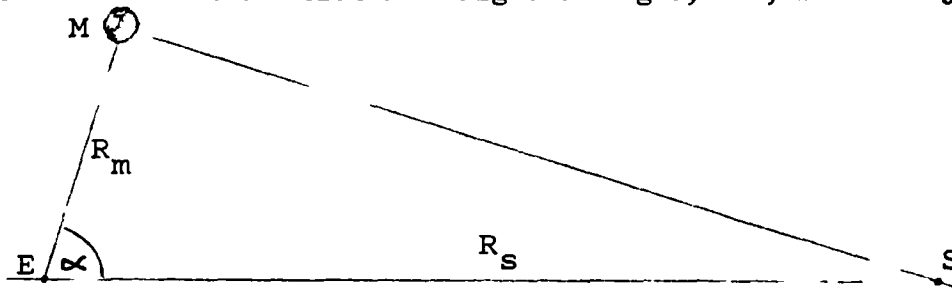
2. Ibid., p.271.



obtained. Being unable to perform exact calculations, Aristarchus apparently resolved to locate the sought ratios within upper and lower bounds.<sup>1</sup>

Specifically, his combined observations and calculations yielded the result:  $18R_m < R_s < 20R_m$ , which is in fact a direct consequence of the inequalities,  $1/20 < \cos 87^\circ < 1/18$ . These inequalities are indeed correct, but the upper and lower bounds are obviously of mathematical origin and do not reflect therefore any physical consideration.<sup>2</sup>

Theoretically, the problem Aristarchus attempted to solve is quite simple once the construction of a right triangle, EMS, has been justified.



The problem comprises the solution of this triangle, in particular,  $R_m/R_s = \cos \alpha$ . If one were to calculate, as indeed Aristarchus did, that  $C_2 < \cos \alpha < C_1$ , then one would get the result,  $C_1 R_m < R_s < C_2 R_m$  ( $C_1$  and  $C_2$  being constant).<sup>3</sup>

In contrast to the theoretical simplicity of this problem, the practical difficulties are enormous. The measurement of the elongation  $\alpha$  at the moment of dichotomy – that is, the moment when the moon is half illuminated – is fundamental to this calculation. However, as Neugebauer remarks, such a measurement 'is totally impracticable'.<sup>4</sup> Since the elongation of the moon changes  $1^\circ$  in about two hours, it is desirable to establish the moment of dichotomy within at least one hour. However, one would consider oneself lucky to determine the night in which dichotomy occurs. In fact,

1. Ibid., p.328.

2. Ibid., pp.333-34.

3. Ibid. Cf., Neugebauer, 1975, pp.634-43.

4. Neugebauer, ibid., p.642.

it seems that the magnitude Aristarchus assigned to the elongation  $\alpha$ , that is  $87^\circ$ , is completely fictitious (that angle is thought to be  $89^\circ 51'$ ).<sup>1</sup>

Moreover, it seems unlikely that the apparent diameter of the moon – a parameter which Aristarchus had to introduce in order to obtain the distances in terms of earth radii – was the result of a direct measurement. One may speculate that any attempt to measure it would have given Aristarchus a better estimate than the  $2^\circ$  which he used. In fact, it appears that Aristarchus himself knew that  $2^\circ$  is a gross overestimate; for Archimedes (286–212 B.C.) reports in his treatise the Sand-Reckoner, that 'Aristarchus discovered that the sun's apparent size is about one 720th part of the zodiac circle',<sup>2</sup> that is  $\frac{1}{2}^\circ$ . As one of the physical assumptions in Aristarchus' calculation is that the moon and the sun are of equal apparent diameter, it seems strange that he did not use that value in his calculations, or amend them in the light of his new observational result.

However, there would have been no surprise if one were to view Aristarchus' treatise as a purely mathematical exercise.

'If Aristarchus chose for the apparent diameter of the sun a value which he knew to be false, it is clear,' Tannery commented in 1883 that this 'treatise was mainly intended to give a specimen of calculations which require to be made on the basis of more exact experimental observations, and to show at the same time that, for the solution of the problem, one of the data could be chosen almost arbitrarily. He secured himself in this way against certain objections which might have been raised.'<sup>3</sup>

Tannery seems to suggest that Aristarchus, being dissatisfied with the

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1. Ibid. Boyer describes Aristarchus' method as unimpeachable; 'the result,' he writes, 'being vitiated only by the error of observation in measuring the angle MES as  $87^\circ$ .' (Boyer, 1968, p.177.) By disregarding the enormous practical difficulty which the measurement of angle MES involves, Boyer misses a crucial element of this method of Aristarchus, namely, that for all intents and purposes, Aristarchus' measurement is a mathematical exercise. Cf., Lloyd, 1982, p.153.
  2. Heath, 1913, p.311.
  3. Quoted by Heath, *ibid.*, pp.311–12.

quality of the physical parameters, proceeded to illustrate his method with an arbitrary numerical value for the apparent diameter of the sun.

Whether or not Aristarchus envisaged much more exact observation and thus, by implication, knew the importance of securing accurate astronomical parameters, cannot be historically established. However, as Neugebauer holds, it is certainly the case that Aristarchus' 'measurement' of the sizes and distances of the sun and the moon 'has as little to do with practical astronomy as Archimedes' Sand-Reckoner in which he demonstrates the capability of mathematics of giving numerically definite estimates even for such questions as the ratio of the volume of the universe to the volume of a grain of sand'.<sup>1</sup> In his treatise, Aristarchus appears to assume numerical data which are, in Neugebauer's words, 'nothing but arithmetically convenient parameters, chosen without any consideration for observational facts', and proceeds to elaborate a pedantic mathematical formulation which is 'unrelated to the complexities of empirical data'.<sup>2</sup> Aristarchus, in other words, treats astronomy as a field of study which, like geometry, sets problems to be solved; the hallmark of Plato cannot here be ignored.

Although Archimedes develops in his Sand-Reckoner, like Aristarchus before him, a pedantic and rigorous mathematical demonstration while ignoring the practical significance of the problem, he does introduce some practical innovations which indicate a certain concern with physical and technical aspects, in order to secure correct data. However, this new perspective does not in itself indicate a substantial divergence from the trend of early Greek astronomy to which Aristarchus' method belongs.

According to Archimedes' formulation in the Sand-Reckoner, the problem of establishing the volume of the universe requires one physical parameter which has to be secured through observation, that is, the apparent solar diameter. To this end Archimedes contrived a dioptra of which he gives

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1. Neugebauer, 1975, p.643.

2. Ibid.

only a sketchy description. It operates with a small vertical cylinder which can be moved on a horizontal ruler into a position which covers exactly the solar disk at sunrise. In addition, he experimented with two very small cylinders in order to determine the width of the observer's pupil.<sup>1</sup> Thus, it appears that Archimedes was not satisfied with the traditional geometrical optics, and tried to combine it with a result from physiological optics. As Neugebauer reports,

the apparent diameter of the sun is then measured as the angle between two tangents to the first mentioned cylinder and the little space which corresponds to the width of the pupil determined in the second experiment.<sup>2</sup>

Archimedes discloses that in his own attempts to determine by means of instruments the angle subtended by the sun, he realized that

this angle is not easy to determine precisely because neither eyes nor hands nor the instruments necessary for the determination are sufficiently free from error to render it exact. But as this point has been frequently made, it is hardly appropriate to discuss it further at this time.<sup>3</sup>

However, neither Archimedes' writings nor any other early work which has been preserved bear this point out. The question as to whether this revealing remark concerning actual practice was so common a point as not to be worth pursuing in the above context, should therefore remain open.

Archimedes found that the angle subtended by the sun's diameter is between  $1/64$ th and  $1/200$ th part of a right angle.<sup>4</sup> On the basis of this result Archimedes proves that the diameter of the sun is greater than the side of a chiliagon (a regular polygon with 1000 sides) inscribed in its orbit. In this proof Archimedes abandons the traditional view that the earth is a point in relation to the sphere in which the sun moves (Aristarchus considered the earth a point even with respect to the sphere in which the moon moves). Archimedes thus indicates his awareness of the phenomenon

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1. Ibid., p.647. Cf., Lloyd, 1982, p.136.

2. Neugebauer, *ibid.*

3. Quoted by Palter, 1970, p.121.

4. Neugebauer, 1975, p.644.

of parallax in the case of the sun.<sup>1</sup>

However, these careful and subtle considerations stand in stark contrast to Archimedes' employment of crude roundings which are perfectly justified in view of his sole objective: to obtain a secure upper bound for the volume of the universe. He, for instance, multiplies the commonly accepted circumference of the earth by a factor of 10; he also more than doubles the diameter of the sun in relation to the diameter of the moon, and he replaces a regular polygon of 812 sides by a 1000-gon.<sup>2</sup>

'And yet,' as Neugebauer remarks, Archimedes 'undertakes a rigorous geometric discussion about the change of an angle observed from the earth's surface when shifted to the center of the earth,'<sup>3</sup> not to mention the measurement of the width of the observer's pupil. To amplify the accuracy of only some parts of the calculation which is, as a whole, based on crude roundings, does not render it more accurate. On the contrary, it shows a lack of insight into the relationship between the abstract power of mathematics and the practice of physics. The width of the observer's pupil and the phenomenon of parallax are, from the standpoint of the degree of accuracy demanded by the problem, simply irrelevant to Archimedes' result that the volume of the universe contains less than  $10^{51}$  grains of sand.<sup>4</sup> This kind of excessive rigour is essentially erroneous; it may be called erroneous rigour - a phenomenon which is, to be sure, not rare in the present time.

A new insight into the interplay between theory and practice, between mathematics and physics, and, moreover, a recognition of the limitation

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1. Heath, 1913, p.348. Heath suggests that Archimedes was the first to recognize the phenomenon of parallax with respect to the sun. (Ibid.) Neugebauer, on his part, maintains that we do not know who introduced the concept of parallax into Greek astronomy. (Neugebauer, 1975, p.322.)

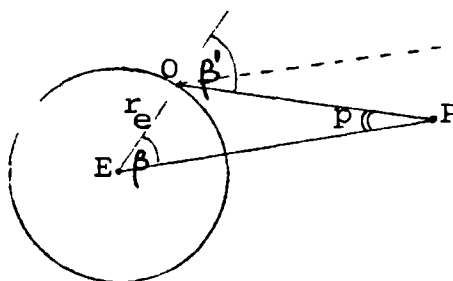
2. Neugebauer, *ibid.*, p.644.

3. *Ibid.*

4. *Ibid.*, p.646. Cf., Lloyd, 1982, pp.153-55.

of knowledge and its pitfalls, that is, errors, are displayed in Hipparchus' work (190 -125 B.C.). I shall specifically outline his attempt to obtain - exploiting the phenomenon of parallax - the distances of the sun and the moon, and his great discovery of the precession of the equinoxes.

In his attempt to determine the distances of the sun and the moon, Hipparchus distinguished, it seems for the first time, between the theoretical and the practical aspect of the phenomenon of parallax. From the theoretical point of view, the problem of parallax is very simple and straightforward: if  $\beta$  is the zenith distance of P, a celestial object, with reference to the point E, the centre of the earth, an observer in O will find a zenith distance  $\beta' > \beta$ . The difference  $p = \beta' - \beta$  is the parallax of P.



The phenomenon of parallax is intimately related to the problem of determining the distances of celestial objects; for if  $\beta$  and  $p$  were known one could compute the ratio of  $EO = r_e$  and  $EP$ ; in other words, one could find the geocentric distance of P measured in earth radii. However, the practical aspect of this phenomenon, that is, the measurement of  $p$ , is anything but straightforward:  $p$  being so small that errors of observation were bound to prevail to a great extent in view of the observational techniques available in antiquity.<sup>1</sup>

I suggest that the recognition of this distinction between theory and practice constitutes a turning point in Greek astronomy, indeed in science, with regard to the awareness of possible observational, and for that matter experimental, errors. However, with the advent of the Ptolemaic

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1. Neugebauer, *ibid.*, pp.100, 1235, Fig.92.

system and its powerful mathematical description, this insight of Hipparchus was lost.

In view of the observational difficulties, Hipparchus, it appears, resolved to find limits within which the solar parallax must lie in order to get observation and calculation to agree in the case of solar eclipse.

G.J. Toomer summarizes Hipparchus' procedure as follows:

'Starting from the fact that there is no observable solar parallax, in "On Sizes and Distances" Bk.1 he [Hipparchus] took the extreme situation, assuming that the solar parallax was zero, that is that the sun was (for practical purposes) infinitely distant. Then using the data from the eclipse of -189 March 14,<sup>1</sup> he derived a minimum distance of the moon (71 earth radii at least distance). However, he was well aware of the unreliability of his premisses: for first, the fact that no solar parallax could be observed did not mean that the parallax was in fact zero; secondly, a small change, to five-sixths or three quarters, in the figure for the size of the eclipse at Alexandria, would cause an increase or decrease of 20% in the resulting lunar distance. Hence his words at the end of the book take on significance: ["In this work we have carried our demonstrations up to this point. But do not suppose that the question of the moon's distance has been thoroughly examined yet. For there remains some matter for investigation in this subject too, by means of which] the moon's distance will be shown to be less than what we have just computed"<sup>2</sup> (in spite of the fact that what he had just computed was a minimum). In Bk.2 he assumed that the solar parallax was the maximum possible, namely 7', and hence computed the sun's minimum distance and the corresponding maximum distance of the moon... the latter being  $67\frac{1}{3} r_e$  in the mean. He then showed that as the sun's distance increased, the moon's decreased towards a limit of 59 earth radii, and was thus able to establish the moon's distance between quite close limits.'<sup>3</sup>

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1. Pappus notes in his account of Hipparchus' procedure that Hipparchus 'takes the following observation: an eclipse of the sun, which in the Hellespontine region was an exact eclipse of the whole sun, such that no part of it was visible, but at Alexandria by Egypt approximately four-fifths of the diameter was eclipsed'. (Quoted by Toomer, 1974, pp.126-27.)
  2. Pappus, *ibid.*
  3. Toomer, 1974, p.139. Cf., Neugebauer, 1975, pp.109-12, 327-29.

The problem of finding accurately the distance of the sun and, as a consequence, its actual size was altogether beyond the instrumental means of astronomers until the invention of the telescope.<sup>1</sup> However, it is to the credit of Hipparchus that he attacked the problem from, so to speak, both ends; a method which enabled him to establish not mathematical but rather physical limits for the value sought. He, furthermore, acknowledged the indefinite nature of his measurements to the effect that the problem remained unsolved. This acknowledgement indicates not only an insight into the roles of theory and practice, but also a scientific honesty, for Hipparchus did not erase his conflicting results. He disclosed that his 'maximum distance' in Book 2, that is,  $67\frac{1}{2}r_e$ , had turned out to be smaller than his 'minimum distance' in Book 1, that is,  $71r_e$ ; these values are nevertheless of the same order of magnitude, and – for the first time in the history of astronomy – in the right region.<sup>2</sup> As Toomer remarks, this kind of openness is rare; it can also be found in the works of Kepler.<sup>3</sup> Kepler, in fact, intended to entitle his planned systematic treatise on astronomy – a treatise similar in its comprehensive goal to the Syntaxis of Ptolemy – by the name Hipparchus, in honour of this great astronomer.<sup>4</sup>

In Toomer's view, Hipparchus' treatise 'is a model of the use of a few observations to squeeze out a reliable result, while retaining due distrust of the accuracy of the observations'.<sup>5</sup>

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1. Dryer, 1953, p.184. Neugebauer remarks that 'it is not surprising that the early attempts at determining the size and distance of sun and moon in relation to the earth ended with wrong results. The ancient methods are of necessity based on trigonometric arguments in combination with visual estimates of very small angles and one naturally had the tendency to falsify such estimates in the wrong direction.' (Neugebauer, 1975, p.634.)
  2. Toomer, 1974, pp.139-40. Dreyer, *ibid.*
  3. Toomer, *ibid.*
  4. However, Kepler did not carry out his plan and wrote instead an elementary text-book of astronomy, Epitome Astronomiae Copernicanae. (Dreyer, 1953, p.403.)
  5. Toomer, *op.cit.*



One source of error in Hipparchus' procedure lies in his a priori assumption of a perceptible solar parallax, an hypothesis which Ptolemy (100-170 A.D.) considered highly questionable.<sup>1</sup> 'In the case of the sun it is quite uncertain,' Ptolemy maintains, 'not merely how great a parallax it has, but whether it has any at all.'<sup>2</sup> However, he himself did not improve on it, on the contrary, as will be observed, he made a distinctly retrograde step which fixed an incorrect solar parallax for almost 1500 years.<sup>3</sup> From the practical point of view, Neugebauer remarks that

the exaggerated value of the solar parallax is of little importance [with regard to the theory of eclipses and planetary motion] compared, e.g., to the effects of refraction and to errors of measurement of times and angles.<sup>4</sup>

Nevertheless, with the improvement of observational techniques and the accumulation of observational records, one would have expected that the scale of the planetary system could have been gradually enlarged. But as it happened, it was Kepler who - having amassed really refined observations - realized that a reduction to 1/3 of the incorrect ancient solar parallax should be introduced.<sup>5</sup>

Hipparchus' treatment of the problem of solar parallax does not surpass in its insight, however innovative it is, his great discovery of the

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1. Neugebauer, 1975, p.329.

2. Quoted by Toomer, 1974, p.126.

3. Dreyer, 1953, pp.184-85.

4. Neugebauer, 1975, p.111.

5. Ibid. In Neugebauer's view 'Muslim astronomers... restricted themselves by and large to the most elementary parts of Greek astronomy: refinements in the parameters of the solar motion, and increased accuracy in the determination of the obliquity of the ecliptic and the constant of precession'. (Ibid., p.145.) However, Neugebauer remarks that 'the conceptual elegance of Ptolemy's cinematic models and the logical consistency of the derivation of the fundamental parameters from carefully selected observations made it extremely difficult to introduce more than insignificant modifications of the basic theory'. Thus, Neugebauer continues, 'every attempt at a revision of the foundations of the planetary theory must have appeared, rightly, as a gigantic task, not lightly to be undertaken in view of the consistency of the structure erected in the Almagest'. (Ibid.)

precession of the equinoxes. Babylonian and early Greek astronomy does not distinguish between the sidereal year (the periodic time in which the sun returns to the same position with respect to the fixed stars from whence it departed) and the tropical year (the time interval that elapses between the sun's two successive passages through the same tropic: equinoctial or solstitial point).<sup>1</sup> In other words, this astronomy presupposes the equivalence and constancy of the time intervals which these two distinct ways of describing the periodicity of the sun's motion exhibit. Hipparchus' great discovery is the recognition that the sun returns sooner to the vernal point than to the same fixed star; that is, Hipparchus discovered that the tropical year is shorter than the sidereal year, a discovery which is in effect the discovery of the precession of the equinoxes.<sup>2</sup>

To conceive the possibility of such a distinction requires, first and foremost, a conviction that, as Neugebauer puts it, 'no periodic time interval should be accepted as exactly constant without empirical confirmation through observations distant as far as possible from one another'.<sup>3</sup> Holding to this methodological outlook, Hipparchus scrutinized earlier records of fixed star distances with respect to equinoxes and solstices, and data concerning the moments of equinoxes and solstices (such records, about 150-170 years old, were available to him), and compared these observations with contemporary results he himself had obtained.<sup>4</sup> In performing this comparison Hipparchus exhibits not only an awareness of the importance of accurate empirical observations, but also the ability to carry this understanding into effect, that is, to attempt to evaluate the errors in these observations and thus to assess the validity of these observational

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1. Ibid., pp.54, 369, 529, 543 note no.13, 1082-83.

2. Ibid., pp.807 note no.15, 1082-83.

3. Ibid., p.54.

4. Ibid., pp.292-98.

results.<sup>1</sup> Hipparchus published the results of this attempt in the treatise On the Length of the Year, in which he comes to the conclusion that 'the equinoctial points move at least 1° per century in a direction opposite to the order of the zodiacal signs'.<sup>2</sup>

Ptolemy reports that in assessing the validity of the observational results, Hipparchus realized that errors could easily account for a shift of up to a quarter of a day.<sup>3</sup> Adhering to his methodology, Hipparchus did not exclude a priori the possibility of variations in the lengths of the years: either sidereal or tropical, or both.<sup>4</sup> It was, therefore, a problem for him whether or not these periodic time intervals are constant.

'It is clear... from these observations,' Ptolemy quotes Hipparchus commenting, 'that the differences of the years have been very small. But as regards the solstices I do not despair of my and Archimedes' being in error both in observation and in calculation even up to the fourth part of a day. But the irregularity of the early periods can be accurately apprehended from observations made on the bronze ring set up in Alexandria in the so-called Square Hall.'<sup>5</sup>

Thus, Hipparchus seems to have found that real variations in the length of the tropical year must be admitted, notwithstanding his awareness of possible errors of up to six hours arising from either observations or calculations, or both.<sup>6</sup>

The great achievement of Hipparchus lies in his attempt to assess,

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1. However, see the criticism of Aaboe and Price, particularly the discussion of the different accuracy obtained in solstice and equinox observations. (Aaboe and Price, 1964, pp.6-10.) Cf. *infra*, p.78.
  2. Neugebauer, 1975, p.293. This discovery apparently led Hipparchus to introduce real ecliptic coordinates because longitudes increase proportionally with time whereas latitudes remain unchanged. (Neugebauer, 1969, p.69.)
  3. Lloyd, 1979, p.181 note no.295. Lloyd, 1982, p.141. Neugebauer, 1975, p.294.
  4. Neugebauer, 1975, p.298.
  5. Quoted by Lloyd, 1982, p.141.
  6. Hipparchus adduces another proof for variation in the length of the tropical year from calculations based on eclipse data. However, Ptolemy criticizes this proof and considers it circular. (*Ibid.*, pp.142, 156. Neugebauer, 1975, p.295. *Infra*, p.69 note no.1.)

theoretically as well as practically, earlier observations, that is, to determine their reliability and accuracy, but above all in his conclusion that in spite of what the experimental errors can account for, a new phenomenon has to be acknowledged. Although Hipparchus' conclusion is incorrect - it was Ptolemy who correctly established the constancy of the tropical year<sup>1</sup> - his methodology nevertheless points in the right direction: it does take the problem of experimental error into account in however rudimentary and unsuccessful fashion that may be. Moreover, since he had at his disposal only few observations, neither very old nor very accurate, he formulated his results, as Neugebauer puts it, 'very cautiously and in a preliminary form'.<sup>2</sup> He, for example, questioned the suggestion that the poles of the ecliptic are the centre of the motion of the precession, as he could not demonstrate it from the very limited empirical material that he had; a suggestion which Ptolemy did not doubt any longer.<sup>3</sup>

Characteristically, Hipparchus was aware that his limited data could not support a definite determination of the magnitude of the precession. He thus resolved to set a lower limit and considered it to be 1° per century. This judgement was vindicated later since Hipparchus reports in his later treatise, On the Displacement of the Solstitial and Equinoctial signs, that he 'found Spica to be six degrees from the autumnal equinox, while Timocharis had found the distance to be eight degrees'.<sup>4</sup> Timocharis had observed Spica in 294 and 283 B.C., while Hipparchus observed it in 129 B.C., thus the change amounts to 45" or 46" a year, that is, about  $1\frac{1}{4}^{\circ}$  per century.<sup>5</sup>

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1. Neugebauer, *ibid.* Cf., *infra*, p.68. Copernicus also did not realize that errors of observation were quite sufficient to account for the difference between the various values of the constant of precession. (Dreyer, 1953, p.329.)

2. Neugebauer, *ibid.*, p.294.

3. *Ibid.*, pp.294 note no.15, 296.

4. Dreyer, 1953, p.203.

5. *Ibid.*

Almost two and a half centuries after Hipparchus had introduced the requirements for new standards in astronomical studies, Ptolemy succeeded in casting the observations and calculations into a comprehensive system, namely the Ptolemaic system. There is no doubt that Ptolemy drew from Hipparchus' works, be they methodological, theoretical or observational.<sup>1</sup> Indeed, it seems that Hipparchus had anticipated a Ptolemy who would put his results to use, for he consciously prepared the ground for the emergence of such a system.<sup>2</sup> As Ptolemy writes, it was because he

had not received from his predecessors as many accurate observations as he has left to us, that Hipparchus, who loved truth above everything, only investigated the hypotheses of the sun and moon, proving that it was possible to account perfectly for their revolutions by combinations of circular and uniform motions, while for the five planets... he has not even commenced the theory, and has contented himself with collecting systematically the observations and showing that they did not agree with the hypotheses of the mathematicians of his time.<sup>3</sup>

The great achievement of Hipparchus was not the prediction of future eclipses for 600 years, as Pliny - apparently following a certain tradition - would have us believe,<sup>4</sup> but rather, in Neugebauer's words,

'the arrangement and classification of the material at his disposal from the past 600 years. This meant the laying of a solid foundation for theoretical astronomy and making it possible for Ptolemy to take full advantage of the past in relation to his own observations... without this work,' Neugebauer holds, 'one could never have hoped to predict eclipses with reasonable accuracy and to test the foundations of theoretical astronomy.'<sup>5</sup>

According to Neugebauer, Hipparchus 'was fully conscious of the fact that many of the parameters as well as the theoretical models at his disposal were only approximations in need of refinement by future generations'.<sup>6</sup> In his 'Notes on Hipparchus', Neugebauer concludes, that 'it is our good

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1. E.g., Neugebauer, 1975, p.89.

2. Dreyer, 1953, pp.161, 166-67.

3. Ibid., pp.165-66.

4. See Neugebauer, 1975, pp.319-21.

5. Ibid., p.321.

6. Ibid., p.320.

luck to be able to see in the Almagest how Ptolemy utilized this material with supreme skill'.<sup>1</sup>

The view that Hipparchus sought to establish a sound and solid foundation for astronomy by providing observations and arranging them for proper analysis by future generations,<sup>2</sup> did not escape the perceptive eye of Kepler. He drew the attention of Maestlin, his teacher, to the following parallel:

You can see in what manner God disposes of his gifts; one man cannot do everything. Tycho Brahe has done what Hipparchus did; he has laid the foundations of the edifice, and has accomplished an enormous amount of work. Hipparchus had need of a Ptolemy who built thereon [the theories] of the five planets. I have done as much whilst he [Tycho Brahe] was still alive.<sup>3</sup>

Ptolemy however had not consolidated the methodological and observational achievements of Hipparchus, as much as Kepler did with regard to Tycho Brahe's. Unlike Hipparchus, Ptolemy neither acknowledge the limitations of his results nor did he examine them critically; he did not pursue his studies along an open path but rather saw to it that his system would account for the phenomena. Ptolemy did not make explicit the criteria upon which he judged some observations more accurate and reliable than others. He thereby exposed his methodology, as Lloyd points out, to the charge of circularity: 'the observations are judged accurate because they confirm the theories (Hipparchus' or his own) and the theories are accepted on the grounds that the "best" observations confirmed them'.<sup>4</sup>

Ptolemy, like Hipparchus, determined the lunar distance as 59 earth-radii; but unlike Hipparchus he rendered it exact.<sup>5</sup> Admittedly, this value is in the right region as the accepted value is  $60\frac{1}{3} r_e$ ; however,

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1. Neugebauer, 1956, p.296.

2. Ibid.

3. Quoted by Koyré, 1973, p.398 note no.4. Neugebauer puts it this way: 'One may perhaps say that the role of Apollonius, Hipparchus, and Ptolemy has a parallel in the positions of Copernicus, Brahe, and Kepler.' (Neugebauer, 1975, p.309.)

4. Lloyd, 1982, p.158.

5. Toomer, 1974, p.131.

it appears that Ptolemy's result is approximately right only because, as Toomer explains, 'a series of errors in observation and theory cancel each other'.<sup>1</sup> Endorsing this view, Neugebauer holds that in general 'it is only the accidental interplay of a great number of different inaccuracies of empirical data and of computations that lead to nearly correct results'.<sup>2</sup> But was it accidental? In view of the fact that Ptolemy knew in advance at what value of the lunar distance he should arrive, namely, Hipparchus' result, it seems incredible that this happened fortuitously. In other words, it is not unlikely that Ptolemy selected those observations which he had thought he could manipulate to produce exactly Hipparchus' result and thus to render his own result exact.<sup>3</sup>

This kind of circular procedure in which results are adjusted to tally with the tested theory, was not unheard-of in classical time. In acoustics, for example, results of real or purported experiments are invariably presented, as Lloyd puts it,

in the form of ratios that exactly correspond to what acoustic theory demanded - and they do so even when the tests referred to could not conceivably have yielded anything like those results.<sup>4</sup>

Indeed, Ptolemy himself perfected, so to speak, this circular method of research in his investigation of the phenomenon of refraction which has a great bearing upon the accuracy of astronomical observations. In his Optics, Ptolemy describes detailed experiments to determine the refraction that occurs when light passes from air to water, from air to glass and from

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1. Ibid., p.131 note no.25.

2. Neugebauer, 1975, p.106.

3. Toomer, op.cit. Lloyd suggests that Ptolemy settled on a one-value parameter, instead of a bounded one in order to simplify the computations. (Lloyd, 1982, p.155.) Cf., infra, pp.73-74.

4. Lloyd, 1982, p.151. However, Lloyd points out that 'in acoustics, as in astronomy, it was sometimes recognised that different observers will get different results'. (Ibid., p.132 note no.8.) Indeed, when Plato discusses harmonics in the Republic, he remarks that 'some say they can distinguish a note between two others, which gives them a minimum unit of measurement, while others maintain that there's no difference between the notes in question'. (Plato, 1974, p.340 (530).)

water to glass. The results are set in tables and although some of them are qualified as 'very nearly', they all tally exactly with a general law which however is not stated. To be sure, the law is not correct and it appears that here, as Lloyd puts it, 'the observations have been interpreted before they are recorded'.<sup>1</sup>

It now becomes clear why Ptolemy determined the solar parallax and distance so confidently. For having arrived at what he thought to be the exact lunar distance and thus parallax, he proceeded to calculate the solar parameters and assigned confidently to the parallax the value 2'51". He thereby ignored the cautious methodology of Hipparchus and established an incorrect value, about 19 times too great, which conforms to his world picture of nested planetary orbits: a geocentric model that lasted for almost 1500 years.<sup>2</sup>

Another example is concerned with the determination of the magnitude of the precession of the equinoxes whose discovery is due, it may be recalled, to Hipparchus. This case bears all the traits of the previous one: whereas Hipparchus had determined it to be at least 1° per century, Ptolemy concluded that it is exactly 1°. <sup>3</sup> Hipparchus, however, is not only methodologically correct, but - in view of the accepted value: about 1.4° - also factually correct. Ptolemy's value for the precession produces a deviation of more than 1° in three centuries and thus a noticeable discrepancy would have resulted comparatively soon, if only there had been a careful observer to look for it.<sup>4</sup>

To the credit of Ptolemy it should be noted that he realized that at his disposal were sufficient observations for demonstrating that the

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1. Lloyd, 1982, p.151. Cf., Lloyd, 1979, p.197; Neugebauer, 1975, pp.892-96; Palter, 1970, pp.121-22; Smith, 1982.

2. Neugebauer, 1975, pp.112, 634, 917-22.

3. Toomer, 1974, p.131 note no.25. Cf., Dryer, 1953, p.203; Neugebauer, 1975, pp.54, 160; Lloyd, 1982, pp.147-49.

4. Neugebauer, *ibid.*, pp.986, 1037.



slow motion of precession proceeds about the pole of the ecliptic and not about the pole of the equator.<sup>1</sup> Furthermore, he held that the observations did not confirm fluctuations in the length of the tropical year; thus he considered the amount of precession constant. He argues that

we are sure by the continuous instrumental observations we have made of tropics and equinoxes that these periods [the time between successive tropics or equinoxes] are not unequal. For we find them differing by no appreciable amount from the additional quarter day, but at times by about as much as could be attributed to the error due to the construction or position of the instruments.<sup>2</sup>

In Ptolemy's view a deviation of 6 minutes of arc from the equatorial plane in the position of the instrument, generates an error of 6 hours in the determination of the time of the equinox. Ptolemy in fact considered unreliable the instrument at Alexandria to which Hipparchus had referred.<sup>3</sup>

Although Ptolemy concluded that there is no variation in the tropical year, he admitted that its actual length of time is difficult to determine and he emphasized what Hipparchus had already realized, namely that to determine accurately periods of return, it is necessary to use observations distant in time as far as possible from one another.<sup>4</sup> 'The period of return will be obtained as nearly exactly as possible,' Ptolemy rightly maintains, 'the longer the time between the observations compared.'<sup>5</sup> Moreover, as Lloyd points out, Ptolemy occasionally stated the need to base conclusions on as many observations as possible.<sup>6</sup> And since he realized that alternative

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1. Ibid., p.34.

2. Quoted by Palter, 1970, pp.122-23.

3. Lloyd, 1979, p.181 note no.295; Lloyd, 1982, pp.140-42, 145.

4. Lloyd, 1982, pp.142, 146-7.

5. Quoted by Lloyd, *ibid.*, p.142.

6. Ibid., p.145. However, as Lloyd stresses, it is not in dispute that the paucity of the actual observations cited in Ptolemy's detailed accounts of the movements of the planets in books IX to XI is remarkable. For each planet he cites almost the minimum number of observations that are necessary to determine the parameters of what is after all a complex model. (Lloyd, 1979, p.186.) Ptolemy is in general quite confident that his theories work well; indeed, he considers approximate or uncorrected figures adequate for the exposition of his model. (Ibid., p.187 note no.325.)

methods may be used to obtain the same result - in the case of the determination of the length of the tropical year, he cited both direct observations of solstices and equinoxes and results arrived at indirectly by calculations based on eclipse data<sup>1</sup> - he recommended their use to provide a checking procedure; a very powerful method indeed which Kepler also used.<sup>2</sup>

Though Ptolemy has emerged as the creator of a dogmatic astronomy much enhanced by his mathematical genius, it is none the less a point of fact that, like Archimedes, he too was interested in problems of observation; prominent amongst them in astronomy is the assignment of limits for permissible discrepancy between observation and calculation. It appears that for Ptolemy the limits of tolerance of discrepancy between observation and calculation are 10' of arc.<sup>3</sup> Although this important consideration is only implied, it does indicate that there is after all a methodological difference between his optical and astronomical works. As Lloyd explains,

unlike the Optics, the Syntaxis does not, as a whole, present results that have already been tailored to match the theory precisely. The problem there is not that discrepant observational data are corrected, in a bid to obtain perfect fit with the theories, but rather that they are tolerated - along with a very broad tolerance of other sources of imprecision in the purely mathematical part of the calculations.<sup>4</sup>

Another case in point is Ptolemy's clear grasp of the impossibility of establishing accurately actual - as distinct from relative - planetary distances. Ptolemy explicitly states that the problem of planetary distances could only be solved if direct measurements of all the various

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1. However, Ptolemy criticized Hipparchus' indirect method of determining the length of the tropical year using the data of lunar eclipses. He argued that these calculations presuppose correct determinations of equinoctial points, and cannot be carried out independently of assumptions about the sun's position. Ptolemy exposed thereby the circularity of this method. (Lloyd, 1982, pp.142, 156. Neugebauer, 1975, p.295.)
  2. Lloyd, *ibid.*, p.145.
  3. Dreyer, 1953, p.195; Neugebauer, 1975, p.99; Palter, 1970, p.126; Toomer, 1974, p.129. However, Lloyd points out that Ptolemy does not always set out his workings in such a way that one can see precisely what margin of error he allowed himself. (Lloyd, 1982, p.149.)
  4. Lloyd, *ibid.*, p.152.

parallaxes were available.<sup>1</sup> However, such measurements were at Ptolemy's time impossible, and thus as far as Ptolemy was concerned this problem should have remained unsolved.<sup>2</sup> Notwithstanding this recognition, Ptolemy thought it fit to construct on the basis of his evaluation of the solar distance - which needless to say is erroneous due to incorrect solar parallax obtained indirectly - a planetary shell structure that came to prevail till the advent of the Copernican system. It is worth noting that the description of the apparent planetary motions as projected onto the celestial sphere, does not require actual distances. Ptolemy, it appears, could not resist the temptation, so to speak, and solved the very problem he himself had suspended.<sup>3</sup>

Ptolemy in fact showed that he could be quite critical with regard to practical procedures and the validity of their results. To measure, for example, the apparent diameter of the sun and the moon, one would employ an instrument of the dioptré type. Aristarchus and, later on, Archimedes arrived, it will be recalled, by such a method at one 720th part of the circumference of the zodiac circle and  $\frac{1}{2}^\circ$  respectively; in Ptolemy's time, this was generally accepted as the correct value. Yet, as Neugebauer reports, it became fashionable to embellish this direct measurement with the timing of the rising of the solar disc by means of a water-clock. The claim was that the resultant quantity of water was one 720th of the total daily outflow. To obtain such a result one has to guarantee an accuracy of 1/1000 in the measurement of the daily outflow; a requirement which was then all but impossible. Ptolemy was aware of this folly and exposed its fictitiousness.<sup>4</sup>

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1. Neugebauer, 1975, p.148.

2. Neugebauer, 1969, pp.155-56.

3. Neugebauer, 1975, pp.148, 917-22, 1088; Lloyd, 1979, p.199.

4. Neugebauer, *ibid.*, pp.103, 657-58. Ptolemy in fact adduces an array of arguments against this method: (1) the hole of the clepsydra gets stopped up; (2) the quantity of water that flows out in a night or a day is not necessarily an exact multiple of the quantity taken at the rising; (3) it is inexact to take the chord as equal to the arc it subtends. (Lloyd, 1982, p.143.)

But the most convincing evidence for Ptolemy's interest in practical, in addition to theoretical, problems is his study of optics. Ptolemy did not confine himself to the area of strictly geometrical optics; with his experiments on binocular vision,<sup>1</sup> on the origin of colour sensation (e.g. the mixture of colours on rotating discs),<sup>2</sup> on the refraction of light<sup>3</sup> and on optical illusions (such as the apparent magnification of celestial objects near the horizon),<sup>4</sup> Ptolemy went far into the field of the physiology of sight and optics at large.

In his studies of astronomy and optics, Ptolemy exhibits a great power of analysis and practical inventiveness which rightly makes him one of the greatest figures in the history of science. However, the available historical material does not furnish enough evidence to reach confident conclusions concerning some aspects of his procedures.<sup>5</sup> In his astronomical studies Ptolemy had presumably some other data besides those he quotes, but one remains totally in the dark as to his selective criteria.<sup>6</sup> Lloyd maintains that it is largely a matter of guesswork to determine 'how far he is prepared to adjust his data or to ignore conflicting evidence - how far he systematically biases what he records in favour of preconceived conclusions'.<sup>7</sup> Yet, from the Syntaxis itself, it is abundantly clear, as Lloyd observes, 'that he does not submit his results to rigorous and extensive controls'.<sup>8</sup> In Lloyd's view, 'there can be little doubt that

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1. Neugebauer, 1975, pp.893-94.

2. Ibid., p.894.

3. Supra, pp.66-67. But see infra, p.78.

4. Neugebauer, 1975, p.894. Ptolemy lists in the Optics many illusory phenomena and he attempts to account for them. Far from concluding that sight is deceptive, he stresses the difference between exceptional and normal sight. (Lloyd, 1982, p.161.)

5. Lloyd, *ibid.*, p.147.

6. Ibid., p.150.

7. Lloyd, 1979, p.198.

8. Ibid.

as a whole he sought to confirm earlier results as far as possible, particularly those of Hipparchus'.<sup>1</sup> By giving the minimum number of observations required to determine the parameters and by making adjustments such as discounting minor discrepancies in the reported observational data and roundings in the calculations, Ptolemy weakens the confrontation between theory and empirical results. It is not therefore reassuring that he professes to select the more accurate observations of those recorded by his predecessors, as these are the observations which tend to corroborate his theory.<sup>2</sup> But at the same time, he was prepared, as Lloyd continues to observe, 'to modify the current theory at certain points - to obtain a better fit with such evidence as he had at his disposal'.<sup>3</sup> Lloyd however concludes that the deductive nature of the Syntaxis cannot be disputed: 'it is an exercise in geometrical demonstration and that is where its great strength lies'.<sup>4</sup>

The few cases I have outlined show that in Greek astronomy one can find evidence of some significant concern with the problem of obtaining accurate observational data, though these evidences occur, so to speak, late in the day.<sup>5</sup> Ptolemy was in fact explicitly critical of most ancient

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1. Ibid.

2. Ibid., p.192; Lloyd, 1982, pp.147, 157.

3. Lloyd, 1979, p.198. In his account of Venus, Ptolemy claims that the observational data required the introduction of the equant - or, in one of Ptolemy's expressions, the 'centre for the eccenter which produces the uniform motion'. (Ibid., p.192; Neugebauer, 1975, p.1102.) In Neugebauer's view, the introduction of the equant was an 'important step in the history of the theory of planetary motion..., a step which was eliminated by philosophical reasons in Copernicus' theory but again fully recognized in its importance by Kepler'. (Neugebauer, *ibid.*, p.171.)

4. Lloyd, *ibid.*, p.198.

5. The existence of a Greek star-catalogue of over 1000 stars which gives longitude, latitude and magnitude determinations for each star, is considered another evidence - regardless of the controversy concerning its origin - of sustained observational work. (Ibid., pp.183-84, 200; Dreyer, 1953, pp.202-3; Neugebauer, 1969, pp.68-69; Neugebauer, 1975, pp.53-54, 280-92, 577, 836, 1087; Palter, 1970, p.126.)

observations from which he had to draw on for his planetary theories; in his view they had been recorded 'inattentively and at the same time in a rough and ready fashion'.<sup>1</sup> Indeed, he, and Hipparchus before him, were aware of particular sources of error in observations and calculations.<sup>2</sup>

However, as R. Palter remarks,

repetition of experiments, cross-checks of experimental findings, rigid control over measurement procedures, scrupulous reporting of all measurements: these must have been exceptional, if they occurred at all, in ancient astronomy.<sup>3</sup>

Lloyd concurs with this view in concluding that although it is not doubtful that Ptolemy realized the importance of obtaining trustworthy data, parts of the Syntaxis show, nevertheless,

little awareness of the need for the rigorous and repeated checking and control of results against accumulated evidence - or of the need for the meticulous recording and presentation of that evidence.<sup>4</sup>

To be sure, here one has to guard against the historical mistake of passing a negative judgement on the standard of ancient observations in the light of the requirements of modern procedures; yet, there is significant evidence of retrograde steps: e.g., the abandonment of available proper methods and the disregard of glaring discrepancies.

One such proper method which is very suitable to handling inaccurate data, namely the bracketing of a measurement result between upper and lower bounds, was never put to extensive use in Greek astronomy. This method which had originated in Greek mathematics and gained a proper physical basis in the work of Hipparchus, apparently lost its appeal once a comprehensive system became available. Lloyd suggests that the - or rather, as he adds, a - reason for this step is that the use of this method would have resulted in such complex computations that they would have become

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1. Quoted by Lloyd, 1982, p.133.

2. Ibid., pp.156-57; Lloyd, 1979, p.182.

3. Palter, 1970, p.122.

4. Lloyd, 1979, p.200.

quite unmanageable.<sup>1</sup> It is indeed easier to operate with a parameter to which one value has been assigned rather than a dual one. However, it is one thing to consider such one-value parameter a tentative quantity for the purpose of calculation, and quite another to consider it exact.

The lunar theory which had originated in Hipparchus' work and was later developed by Ptolemy, demands excessive variations of the moon's distance, and thereby of its apparent diameter.<sup>2</sup> However, this expected phenomenon never occurs in reality, and as J.L.E. Dreyer maintains, 'it cannot possibly have escaped Hipparchus and Ptolemy';<sup>3</sup> yet, they took no notice of it.<sup>4</sup> In Dreyer's view, this lack of consideration for a glaring discrepancy between the observed phenomenon and the result of a theory, shows that Hipparchus and Ptolemy 'did not look upon their work as a real system of the world, but merely as an aid to computation'.<sup>5</sup> Dreyer argues that Ptolemy's epicyclic theory 'was merely a means of calculating the apparent places of the planets without pretending to represent the true system of the world, and it certainly fulfilled its object satisfactorily, and, from a mathematical point of view, in a very elegant manner'.<sup>6</sup> Indeed, Ptolemy generally begins the theory of a particular aspect of a planet's motion by saying 'let us imagine... a circle'.<sup>7</sup> Even in the

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1. Lloyd, 1982, p.155. Thus Lloyd holds that the deductive articulation of Ptolemy's theories has effectively ruled out in most cases the use of upper and lower limits for the main fundamental parameters. (Ibid., p.156.) Neugebauer on his part observes that Ptolemy 'resorted to mere approximations when higher accuracy implied too heavy a burden of numerical computations'. (Neugebauer, 1975, p.145.)
  2. The epicycle-eccentric model of Hipparchus and Ptolemy for the sun and moon has been hailed as 'the outstanding example, from the ancient world, of a theory that combined the mathematical rigour the Greek scientists demanded with a detailed empirical base'. (Lloyd, 1979, p.200.)
  3. Dreyer, 1953, p.201. Ptolemy indeed records his awareness of this discrepancy. (Lloyd, 1982, p.139.)
  4. Dreyer, *ibid.*, p.196.
  5. *Ibid.*, p.201.
  6. *Ibid.*, p.196.
  7. *Ibid.*, p.201.

Planetary Hypotheses - Ptolemy's other astronomical treatise in which he attempted to establish a true physical account of planetary motions<sup>1</sup> - Ptolemy admits in the introduction that 'I do not profess to be able thus to account for all the motions at the same time; but I shall show that each by itself is well explained in its proper hypothesis'.<sup>2</sup> This admission of Ptolemy strengthens the view that Ptolemy's system is not really a system, let alone a physical system, but rather a string of mathematical hypotheses.<sup>3</sup>

It seems safe to conclude that in Greek astronomy the context in which observational results were incorporated into theories is not that of testing but rather corroborating. In their paper which bears the significant title, 'Qualitative Measurement in Antiquity', A. Aaboe and D.J. de Solla Price arrive at the conclusion that

the role of instruments in antiquity was to serve convenience rather than precision, and that the characteristic type of measurement depended not on instrumental perfection but on the correct choice of crucial phenomena. If such phenomena could be welded together in a matrix of mathematics, the agreement between observation and theory was perfect. Needless to say, if one phenomenon did not fit, it had to be rejected as inaccurate and imperfect.<sup>4</sup>

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1. Lloyd, 1979, p.199; Neugebauer, 1975, pp.900ff.

2. Quoted by Dreyer, op.cit.

3. The phenomenon of annular solar eclipse is another case in point. As Ptolemy assumed that the apparent lunar diameter equals the apparent solar diameter when the moon is at its maximum geocentric distance (previous astronomers had assumed equality for the moon at mean distance), he in effect denied the possibility of annular solar eclipse. However, in all probability such a phenomenon was observed still in his lifetime. But, as Neugebauer remarks, 'neither then nor during the next 1400 years was the obviously necessary modification... undertaken'. (Neugebauer, 1975, pp.104, 111.) Kepler studied carefully reports of such an eclipse and considered them correct. (Infra, pp.127-28. In general, Kepler did not rest until he was able to reconcile all aspects of theory and observations, whereas Ptolemy's theory had been accepted for centuries without any attempt to eliminate its defects. (Neugebauer, 1975, p.98.) 'I have built up a theory of Mars', Kepler writes to his teacher, Maestlin, 'such that there is no difficulty about agreement between calculation and the accuracy of observational data'. (Quoted by Koyré, 1973, p.397 note no.4.)

4. Aaboe and Price, 1964, pp.3-4.



Thus, when Neugebauer writes that 'the ancient astronomers rightly had greater confidence in the accuracy of their mathematical theory than in their instruments',<sup>1</sup> he appears to be giving these astronomers undue credit as this judgement implies that they fully appreciated problems of observation - particularly the limitations of their instruments - and the interplay that exists between mathematical theories and observations. But as Aaboe and Price show, the accuracy which ancient astronomers sometimes present through complicated numbers is in fact an illusion; this illusion of accuracy is the result of crude numerical data having been fed into the mathematical machinery.<sup>2</sup> 'Far from any march towards precision by way of instrumental improvement in antiquity, we find,' write Aaboe and Price, 'but a predominant concern with the mathematical niceties of such theory'.<sup>3</sup>

In sum, elaboration of theory, particularly its mathematical basis and geometrical constructs, constituted the central concern of the Greek astronomers. However, this preoccupation with theory at the expense of observation should not be construed as signifying an insight into the limitations of the available observational techniques. Rather, it is an indication that observations played, so to speak, a secondary role: they were used mainly as illustrations, not as a means of testing.

In view of this analysis of Greek astronomy, my principal argument can be formulated thus: in a science where empirical results are more often used for the purpose of illustrating and supporting theories rather

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1. Neugebauer, 1969, p.185. Elsewhere Neugebauer writes that 'it makes no sense to praise or to condemn the ancients for the accuracy or for the errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure, erected in spite of the enormous difficulties that beset the attempts to obtain reliable empirical data'. (Neugebauer, 1975, p.108.)
  2. Aaboe and Price go on to say that 'the simple numbers however produce results that agree remarkably well with the facts, so that we must marvel at the way in which the choice and simple numbers were injected into suitably interlocking chains'. (Aaboe and Price, 1964, p.20.)
  3. Ibid.

than testing them,<sup>1</sup> one would not expect a clear grasp of the concept of experimental error. To be sure, one cannot really deny that the Greek astronomers had some implicit notion of the problem of experimental error. However, this implicit notion did not develop into explicit methodological procedures intended to be applied rigorously to account for experimental errors; rather, it remained stagnant, if it did not regress. Again, in an astronomy where the observations, as Lloyd puts it, 'are cited to illustrate and support particular doctrines'; where 'the observations are sometimes already interpreted in the light of the theories they were meant to establish', and where the support is in many cases exaggerated:<sup>2</sup> in such an astronomy there is no room for a fully developed concept of experimental error. This concept can emerge only in a context where the empirical results are given their due weight as a means of testing.

I shall presently argue that in Kepler one can find a juncture of ideas which was conducive to this proper understanding of the role of empirical results and their evaluation. This juncture, I shall argue, occurred not only because of the pioneering work of Tycho Brahe who had heralded the modern method of observation - the continuous observation to the point of overdetermining the phenomenon<sup>3</sup> - but also because of the genius of Kepler in which the idea of unity was combined with the belief in physical realism.

Before I proceed to discuss the second stage of the focusing process on the problem of experimental error, it is worthwhile to note a distinction which Lloyd has drawn to facilitate the discussion on errors of observation. In his paper on observational error in later Greek science, Lloyd distinguishes, as he puts it, 'between problems that arise from the conditions under which the object is to be observed or from the nature of the object

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1. Lloyd, 1979, p.200.

2. Ibid., p.221.

3. Aaboe and Price, 1964, p.16; Neugebauer, 1975, p.1089.

itself, and those that relate to the means or method of observation'.<sup>1</sup>

Lloyd is aware that the distinction is broad and cannot always hold firmly.<sup>2</sup>

Under the first category, that is, the category of conditions of observation, Lloyd includes any type of interference arising from atmospheric conditions.<sup>3</sup> Ptolemy, for example, suggests that

the same angular distances appear greater to the eye near the horizon, and less near the zenith, and so for this reason it is clear that they can be measured sometimes as greater and sometimes as less than the real angular distance.<sup>4</sup>

However, it is worth noting that although Ptolemy recognized and indeed studied the phenomenon of refraction, he does not make any systematic correction to accommodate this hindrance.<sup>5</sup> As an example for problems which arise in the conditions created by the object itself, Lloyd suggests the case of determining solstices and equinoxes. At a solstitial point, the sun is either at its maximum or minimum declination. Two days away from a solstitial point, the sun's declination differs but 1' from the extreme value; and after five days the declination changes by as little as 6'. In contrast, the declination of the sun changes at an equinox by about 24' per day. Thus, whereas an equinoctial point can be determined to an accuracy of 1/4 day (allowing for an inaccuracy of 6'), a solstitial point cannot be located directly to an accuracy better than some three or four days.<sup>6</sup> Ptolemy was aware of this difference and indeed remarks on the greater accuracy of equinox observations.<sup>7</sup>

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1. Lloyd, 1982, p.133.

2. Ibid., pp.133-34.

3. Ibid., pp.134-35.

4. Quoted by Lloyd, *ibid.*, p.135; cf., *ibid.*, note no. 12.

5. Ibid., p.134. However, as Neugebauer remarks, 'it should be remembered how difficult the problem still appeared to Brahe and Kepler when it was taken up around 1600'. (Neugebauer, 1975, p.896.)

6. Aaboe and Price, 1964, p.9.

7. Lloyd, 1982, p.135.

The second category consists of problems which arise in the means and method by which the phenomenon is observed. Here Lloyd includes the use of sighting aids or other instruments.<sup>1</sup> The experiments of Archimedes on the dioptra as reported in the Sand-Reckoner, constitute such a case. Another example is Ptolemy's rejection of the unsound attempt to establish the apparent diameter of the sun or moon by means of the water-clock.

This broad distinction of Lloyd constitutes a preliminary stage towards a general classification of experimental error. Although the distinction is intended to account only for observational errors, the motivation is the same: to clarify the problem of experimental error one may classify different types of errors which arise in different contexts; or, to put it in already familiar terms, one may distinguish between the different ways in which something 'turns out wrong'. Lloyd classifies, as we have seen, two types of observational errors: those that are associated with either the external or internal conditions of the observed object, and those that pertain to a particular method of observation. In the light of my discussion on Kepler's awareness of the problem of experimental error, it will become apparent that a general and yet refined classification - more than the one Lloyd applies to Greek astronomy - is needed to truly reflect Kepler's understanding of the problem.

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1. Ibid., pp.136ff.

## CHAPTER III

### Kepler's View of the Concept of Experimental Error

In 1605, Bacon published The Advancement of Learning which was metaphorically - in the spirit of the contemporaneous great geographical discoveries - a chart of the lands already discovered and known. With this chart, Bacon intended to direct the attention of the 'prospector of knowledge', without loss of time or labour, to those parts which had not yet been explored. Fifteen years later, he inaugurated a new method, an instrument - Novum Organum - by means of which men should arrive at these novelties.<sup>1</sup>

Novum Organum, it may be recalled, starts with an examination of types of error which Bacon calls idols. Under the first category - namely, the idols of the tribe - Bacon subsumes, as the source of these idols, the peculiar nature of the human intellect. Due to its peculiar nature, the human intellect, according to Bacon, 'easily supposes a greater order and equality in things than it actually finds; and, while there are many things in Nature unique and quite irregular, still it feigns parallels, correspondents, and relations which have no existence'.<sup>2</sup> Bacon considers what he calls a fiction, namely '"that among the heavenly bodies all motion takes place by perfect circles", spirals and eccentrics being altogether rejected',<sup>3</sup> an appropriate illustration of this first category of idols.

However, the origin of this category and its illustration can be traced directly to a view Bacon has already expressed in The Advancement of Learning. There he writes 'that the spirit of man, being of an equal and uniform substance, doth usually suppose and feign in nature a greater equality and uniformity than is in truth'.<sup>4</sup> Thus, he argues, 'the mathematicians

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1. Bacon, 1950, pp.viii-ix.

3. Ibid.

2. Bacon, 1859, p.24 (Bk.I, xlv).

4. Bacon, 1950, p.133.

cannot satisfy themselves except they reduce the motions of celestial bodies to perfect circles, rejecting spiral lines, and labouring to be discharged of eccentrics'.<sup>1</sup>

Bacon appears to hold that in the intervening years between the publication of The Advancement of Learning and the inauguration of his new method, nothing substantial has occurred which might have called into question his claim that due to the peculiarity of the human mind, one can satisfy oneself only when planetary motion is shown to take place in a perfect circle. That Bacon did not know in 1605 of Kepler's astronomical work is quite understandable: by that time Kepler had published only one major work, the Mysterium Cosmographicum (1596), and was in the process of completing his investigation into planetary motion. However, by 1620, Kepler had already published, in addition to the youthful Mysterium Cosmographicum, three major astronomical works: Astronomia Nova (completed 1607, published 1609), Epitome Astronomiae Copernicanae (in three parts of which the first volume was published in 1618) and Harmonices Mundi libri v (1619). By 1620, all of Kepler's three laws had seen publication: Bacon's warning against the allurements of the human intellect had been heeded; the promulgation of the laws confuted his illustration.<sup>2</sup>

Essentially, two historiographical traditions can be discerned in

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1. Ibid.

2. Ibid., note no.2. Another unfortunate illustration occurs in Wittgenstein's On Certainty. 'Suppose some adult had told a child that he had been on the moon. The child tells me the story,' Wittgenstein writes in 1950, 'and I say it was only a joke, the man hadn't been on the moon; no one has ever been on the moon; the moon is a long way off and it is impossible to climb up there or fly there.' (Wittgenstein, 1977, p.16e (§106).) Wittgenstein remarks further that 'we all believe that it isn't possible to get to the moon; but there might be people who believe that it is possible and that it sometimes happens. We say: these people do not know a lot that we know. And, let them be never so sure of their belief - they are wrong and we know it. If we compare our system of knowledge with theirs then theirs is evidently the poorer one by far'. (Ibid., p.37e (§286).) So much for Wittgenstein's own system of knowledge.

Kepler studies.<sup>1</sup> One tradition of historiography - the earliest one - crystallized mainly around a nucleus of scientists who were active in the Paris Academy during the last half of the eighteenth century and the early part of the nineteenth century. Much influenced by the French encyclopaedic philosophy, they sought to demonstrate the orderly progress of the human mind and to furnish the practising scientist with a useful repository of ideas, methods and theorems. Thus, they tended to expound and explicate only the technical part of Kepler's work. This tradition culminated in Delambre's History of Modern Astronomy (1821), in which some 300 valuable pages are devoted to the mathematical and empirical aspects of Kepler's scientific work. This early technical tradition, as Westman points out,

'shed much light on Kepler as an ingenious astronomer, the discoverer of new techniques and of new laws of nature. But,' Westman continues, 'it was frequently embarrassed - "surprised and distressed" are Delambre's words - at the mystical, metaphysical reasoning that inexorably permeated the writings of the Great Man.'<sup>2</sup>

This approach tends to perceive two 'Keplers': on the one hand, Kepler the discoverer of the so-called empirical laws of planetary motion, and on the other hand Kepler the mystic whose metaphysics can safely be dismissed. in the words of Laplace, as chimerical speculation.<sup>3</sup> Consider for example Berry's Short History of Astronomy (1898); upon evaluating Kepler's work he writes that the scientific works of Kepler

fill but a small part of [his]... voluminous writings, which are encumbered with masses of wild speculation, of mystic and occult fancies, of astrology, weather prophecies, and the like, which are not only worthless from the standpoint of modern astronomy, but which - unlike many erroneous or imperfect speculations - in no way pointed towards the direction in which the science was next to make progress, and must have appeared almost as unsound to sober-minded contemporaries like Galileo as to us.

And Berry concludes that 'if Kepler had burnt three-quarters of what he printed, we should in all probability have formed a higher opinion of his intellectual grasp'.<sup>4</sup> As we shall see, this view, if somewhat less strongly

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1. Westman, 1975, pp.58-63.

3. Aiton, 1976, p.77.

2. Ibid., p.59.

4. Berry, 1961, p.197.

expressed, has never subsided.

In reaction to this tradition, Westman explains, there developed a second historiographical trend; it focused attention away from the explanation of Kepler's astronomical techniques qua techniques and, instead, demanded an investigation of the philosophical foundations of his discoveries.<sup>1</sup> It was Whewell (1794-1866) who first articulated this viewpoint, claiming that 'in making many conjectures, which on trial proved erroneous, Kepler was no more fanciful and unphilosophical than other discoverers have been'.<sup>2</sup> But the most influential analysis of Kepler's writings is due to Cassirer (1874-1945) who, in The Problem of Knowledge (1906), ushered in a new era of Kepler studies, and indeed established this second historiographical approach.

Underlying Cassirer's analysis is the Kantian claim that reality as the object of experience cannot be reached, as for example Plato's ideal entities; reality - as it is perceived - is rather conditioned by certain intuitive functions of the mind. Thus, the concepts of science are, as Cassirer puts it, 'symbols of the ordering and connecting functions which present the inner nature of the concrete' to the mind. For Cassirer, therefore, science is not a cumulative process but rather a reflection of the creative features of the mind in shaping and impressing, in each epoch, its own conceptions on reality.<sup>3</sup>

A recurring issue in Kantian philosophy is the problem of unity. Knowing for Kant involves the faculties of sensuous intuition, imagination and understanding, which are combined in the unity of the subjective consciousness. Hence, it is no wonder that Cassirer, by extension, examined Kepler's work from the standpoint of the concept of unity; indeed, as Westman points out, 'Cassirer's main achievement...was to demonstrate the

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1. Westman, 1975, p.59.

2. Quoted by Westman, *ibid.*, p.60.

3. Quoted by Westman, *ibid.*, pp.61-62.



remarkable unitary nature of Kepler's thought'.<sup>1</sup> The notion of harmony, for example, which is so prevalent in Kepler's writings, is taken to be, in Cassirer's analysis, the instrument of the mind which can lay bare the coherence that underlies the seemingly chaotic nature of matter.<sup>2</sup>

However, it seems to me that Cassirer read too much of his neo-Kantianism into the writings of Kepler. Kepler is widely removed from critical idealism, and his belief in the existence of physical reality does not match, I suggest, the kind of reality Kant's idea of knowledge professes to attain. But this dispute is not here at stake; what is however important to note from the viewpoint of the present subject, is the fact that by introducing the notion of unity into Kepler studies, Cassirer made some headway in the direction of resolving the existence of several 'Keplers' who all lived through the turbulent years between 1571 and 1630. (The Thirty Years' War took place between 1618 and 1648.) Kepler has been described and characterized as a Platonist, Neo-Platonist, Pythagorean, Aristotelian, Occultist, Mystic and the like, and it was the pioneering work of Cassirer which attempted to reconcile all these strands and to present Kepler as a coherent thinker.

The introduction of the concept of unity did not however take firm root in Kepler studies. Consider for example Sarton's view; in his opinion, Kepler lived to his last day in a mist of occultism and his writings, to use Sarton's own words,

'are almost repulsive by their prolixity, obscurity and mysticism. They contain some treasures of inestimable value,' Sarton admits, but continues to ask, 'who will have the courage to look for them in the enormous mass of verbiage wherein Kepler chose to bury them?'<sup>3</sup>

And Eddington concurred when - speaking at the Kepler monument in his birth-place, Weil-der-Stadt, on the tercentenary of his death - he expressed

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1. Ibid., p.62.

3. Sarton, 1922.

2. Ibid.

the view that Kepler was a strange erratic genius whose half fantastic way of thinking, capable of preposterous misjudgement, is scarcely such as one should extol as an example to be generally imitated.<sup>1</sup>

Holton, to take another example, notes the incongruous elements which, in his view, comprise Kepler's work: 'physics and metaphysics, astronomy and astrology, geometry and theology'. He then sets himself the task, as he puts it, of identifying 'those disparate elements and to show that in fact much of Kepler's strength stems from their juxtaposition'. Thus, when Kepler's 'physics fails him metaphysics comes to rescue; when a mechanical model breaks down as a tool of explanation, a mathematical model takes over; and at its boundary in turn stands a theological axiom'.<sup>2</sup> This explains, Holton believes, Kepler's ability to employ interchangeably a universal physical force, a unifying image of the central sun, and a unifying principle of all-pervading mathematical harmonies. Although Holton concedes that mathematics, physics and metaphysics are in fact inseparable - at least in the case of Kepler's belief in the central role of the sun<sup>3</sup> - he does portray, nevertheless, a Kepler who vacillates between mathematics, physics and metaphysics; an approach which has engendered, according to Holton, three different kinds of universe: the universe of mathematical harmony, the universe as physical machine, and the universe as central theological order.<sup>4</sup>

However, much more disturbing than Holton's view of incongruous elements in Kepler's work are the many 'Keplers' that Koyré has created in his various studies. There one can find Kepler the revolutionary thinker vs. the Kepler who is bound by the Aristotelian tradition;<sup>5</sup> Kepler the great scientist vs. Kepler the poor philosopher;<sup>6</sup> and there is even Kepler the geometer vs. Kepler the physicist.<sup>7</sup> Koyré's attempt - in his book,

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1. Eddington, 1930.

2. Holton, 1975, pp.69-70.

3. Ibid., p.81.

4. Ibid., pp.76-78, 86.

5. Koyré, 1974, p.87.

6. Koyré, 1978, p.144; cf.p.146.

7. Koyré, 1973, p.271.

La Révolution Astronomique (1961) - to bring together the two historiographical traditions of Kepler studies is admirable in itself, but however scholarly and informative the book may be it has failed, in my view, to bring to the fore the concept which I claim is crucial to the understanding of Kepler's work, namely, the concept of unity.

I suggest that in the spirit of the Renaissance, and following the works of Nicholas of Cusa (1401-1464) and Pico della Mirandola (1463-1494) in particular,<sup>1</sup> Kepler attempted to achieve unity of knowledge by reconciling the various contemporary philosophies, and to attain thereby what Pico had called pax philosophica.<sup>2</sup> Pico's plan to execute a work by the title Symphonia Platonis et Aristotelis - of which nothing has been preserved - was in a sense carried out by Kepler. I suggest further that the concept of experimental error comes to the fore when one attempts to reconcile the doctrine of Plato with that of Aristotle: abstract forms, of which, in Plato's doctrine, knowledge can be had, do not exist in separation from their manifestations in the concrete world, namely the particulars, to which Aristotle addresses himself. To perceive form and matter as a unity is, in my view, an essential stage towards a better understanding of the concept of experimental error, since this concept comprises intertwined elements of conceptual and physical origins.

It is in this sense that the concept of unity is of prime importance to the understanding of Kepler's awareness of the concept of experimental error. I disagree with Cassirer when he plays down the importance of Kepler's belief in unity by claiming that 'mathematical physics first seeks to provide its claims and independence by going back from the philosophy of Aristotle to that of Plato. Above all,' Cassirer holds, 'it is Kepler

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1. Reference to Cusa and Pico can be found in the 'Personenregister' of Kepler, 1938-1975.

2. Cassirer, 1942, p.126; Garin, 1972, pp.202, 207; Dulles, 1941, pp.xi, 53, 58, 103, 132-34.

of whom this reversion is characteristic'.<sup>1</sup> Kepler did not pursue a reversion; rather, he sought unity, and this is indeed the essence of the difference between him and Galileo.

Kepler states, in 1608, in a letter to David Fabricius - an astronomer who was very critical of the Astronomia Nova - that he 'will interweave Copernicus into the revised astronomy and physics, so that either both sciences will perish or both will keep alive'.<sup>2</sup> Ten years later, in the dedication of his book Epitome Astronomiae Copernicanae - a recapitulation of his discoveries recast in the form of a dialogue intended for the general public - Kepler writes explicitly:

'the philosophy, which I represent, however, most of it was discovered by others; though indeed, I do not present it in slavish dependence but have put it together from different authors, so that one can see how each in his own way has acquired parts of the truth.' And Kepler continues to specify; 'I build my whole astronomy,' he declares, 'upon Copernicus' hypotheses concerning the world, upon the observations of Tycho Brahe, and lastly upon the Englishman, William Gilbert's philosophy of magnetism... for me,' he writes, 'there is so much importance in the true doctrine of others or even in correcting the doctrines which are not in every respect well established, that my mind is never at leisure for the game of inventing new doctrines that are contrary to the true.'<sup>3</sup>

That Kepler sought unity and reconciliation can also be seen in his attitude towards the church which was marked at that time by strife and division. As Duhem characteristically puts it, 'Kepler was a Protestant, but deeply religious'.<sup>4</sup> Kepler indicated, however, that he was neither Lutheran nor Calvinist, nor Jesuit, according to their kind.

'It hurts me in my heart,' he writes, 'that the three great factions have amongst them torn the truth so badly that I must gather it piecemeal wherever I can find a piece. But I don't have to pay back in equal coin. I rather work hard to reconcile the parties where I can do it with truth, in order to after all hold it with many of them. This is why others think of me as a mocking bird when I say against them, I hold it in most cases with two parties against the

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1. Cassirer, 1953, p.135.

2. Baumgardt, 1952, p.81.

3. Kepler, 1952, p.850; Baumgardt, *ibid.*, p.122.

4. Duhem, 1969, p.100.

third. But look,' Kepler concludes, 'I like either all three parties, or at least two against the third, in the hope of concord. My opponents, however, all like each only one single party.'<sup>1</sup>

The search for a common denominator thus became a constant motive in Kepler's theological thinking.

In contrast to Galileo who pursued the nature of a phenomenon by isolating it methodologically as well as epistemologically, Kepler saw his task as a God given mission to understand the divine work of creation as a whole; a unifying outlook was therefore imperative. Kepler's work is imbued with the spirit of unity; his belief in it, I suggest, is first and foremost a religious experience which enhances the belief in the existence of ultimate laws, that is, God's laws of creation. Within this religious framework Kepler developed his ideas by attempting to combine the contradictory elements of the concrete and the abstract, that is, physics and mathematics. 'In every physical object, physical laws, numbers and proportions have been laid down by God,'<sup>2</sup> writes Kepler to Herwart - the Bavarian Chancellor - in 1599. Leges corporis, this is the crucial phrase; it is an expression that represents the attempt to view the concrete and the abstract as a unity.<sup>3</sup> 'God wanted us to recognize [these laws],' Kepler writes further to Herwart, 'by creating us after his own image so that we could share in his thoughts.'<sup>4</sup>

But to return to the contrast between Galileo and Kepler. The reader may recall Sarton's judgement that the writings of Kepler are 'almost repulsive by their prolixity, obscurity and mysticism'. Galileo's writings,

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1. Quoted by Gerdes, 1975, p.366.

2. Kepler's words are: 'Deo enim in toto opere corporeo, leges corporis, numeri et proportiones sunt propositae, leges autem lectissimae, et ordinatissimae'. (Kepler, 1938-1975, XIII p.308.) Cf., Baumgardt, 1952, p.50.

3. The concept of unity and its possible attainment constitute two recurring themes in Pico's Oration on the Dignity of Man (1486). (Pico della Mirandola, 1948, pp.223-54; cf., Kristeller's introduction to Pico's Oration, *ibid.*, pp.215-22.)

4. Quoted by Baumgardt, 1952, p.50; Kepler, 1938-1975, XIII p.309.

in contrast, are in Sarton's view 'models of clear, terse, beautiful language; they are amongst the greatest classics of scientific literature'.<sup>1</sup> Rosen concurs with this judgement and suggests that 'the German's [that is, Kepler's] obscurity, prolixity and mysticism were so repulsive to Galileo that he was disinclined to go digging for the nuggets of real gold hidden away in Kepler's heap of dross'.<sup>2</sup> So much so that Galileo had not even referred to Kepler's laws in his dialogue which was published in 1632, two years after Kepler's death.

Panofsky, the art historian, maintains quite rightly that Galileo's failure to mention Kepler's laws is not a mere didactic tool to make the Copernican system easily comprehensible to the general reader; rather, it is a consequence of his deep-seated objection to any violation of the principle of separation, the very principle which dominated his thought. Panofsky finds Galileo haunted by this principle; he loathed complexity, imbalance, and all kinds of conflation.<sup>3</sup> If this view of Galileo is correct, then the essential tension between Kepler and Galileo would be combination vs. separation, or, more generally, unity vs. compartmentation.<sup>4</sup>

Once this difference is borne in mind the contrasting attitudes of Kepler and Galileo towards the problem of experimental error become clearer: when Kepler encountered a small discrepancy between the prediction of a law and the relevant observations he spent years, having checked the observations, in modifying the law until he obtained a satisfactory

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1. Sarton, 1922.

2. Rosen, 1956, p.79.

3. Panofsky, 1956, pp.5-6, 9-10, 13-15.

4. For another aspect of their opposing approaches see Heisenberg, 1958, pp.104-5.

agreement. Galileo, in contrast, relying on his science of accident,<sup>1</sup> tended to ignore discrepancies. In Galileo's physics, or rather astronomy, the planets move inertially with uniform circular motion - a claim that could not be sustained by observations. And his claim that the oscillations of a simple pendulum are isochronous whatever the amplitude, is also utterly wrong. Mach observes that Kepler, in contrast to Galileo who always sought after the very simplest solutions,

'did not quail before the most complicated assumptions, but worked his way, by the constant gradual modification of his original hypothesis, successfully to his goal, as the history of his discovery of the laws of planetary motion fully shows. Most likely,' Mach speculates, 'Kepler

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1. For Aristotle accident means either 'that which attaches to something and can be truly asserted, but neither of necessity nor usually,' (Aristotle, *Metaphysics*, 1963, 1025<sup>a</sup>13) or 'all that attaches to each thing in virtue of itself but is not in <sup>its</sup> essence.' (Ibid., 1025<sup>a</sup>30.) Thus Aristotle distinguishes between two types of accident: the non-regular and the non-essential. Attributes which attach to subjects only in a particular place and at a particular time, and attach to them not because they are these particular subjects in this place and time, are accidents of the first type. For example, cold in the dog-days is accidental but heat is not. Accidents of the second type, the non-essential, are practically mere names (Ibid., 1026<sup>b</sup>14); that is, when attributes belong to a subject and present in its essence, and yet are not of its essence. For example, having its angles equal to two right angles is an accident of the triangle. (This attribute is not an element of the definition of triangle.)

According to Aristotle, there is no science of accidents: 'science is of that which is always or for the most part, but the accidental is in neither of these classes.' (Ibid., 1065<sup>a</sup>4.) Evidently, the causes of the accidental are not of the same kind as the causes of the essential, for otherwise everything would be of necessity and contingency would be abolished. (Ibid., 1065<sup>a</sup>10-15.) The causes of the accidental are in fact also accidental; (Ibid., 1027<sup>a</sup>8.) hence, the nature of the accidental cannot be traced to first principles. Aristotle therefore holds that 'there is no science of such a thing.' (Ibid., 1065<sup>a</sup>3.)

In contrast to Aristotle, Galileo, according to N. Koertge, seeks a science of the accidental; it is indeed essential to the attainment of physical knowledge. (Koertge, 1977.) The process of idealization has detached the physical law from its concrete setting and placed it in a context of ideal conditions which cannot actually materialize; e.g., the motion of a projectile that does not have to overcome the resistance of air. It is because of this gap between the ideal scientific law - a law abstracted from impediments - and the actual physical setting, that we do not find nor expect to find an exact match between the predictions of an ideal law and the relevant observations. In Galileo's view, as Koertge points out, 'accidents can hide and obscure essences, so that it is impossible for the naive observer to discover them. Accidents not only impede and hinder the motion of falling bodies, they also interfere with our discovering and knowing the laws of motion. The scientist must actively deal with them.' (Koertge, *ibid.*, p.389.)

on finding the assumption  $ds/dt=as$  would not work, would have tried a number of others, and among them probably the correct one  $ds/dt=a\sqrt{s}$ .<sup>1</sup>

It is however equally probable, as John L. Russell points out, 'that Kepler, if he had worked on terrestrial mechanics, would have been so worried by errors arising from friction and air resistance that he would never have arrived at the correct law for falling bodies'.<sup>2</sup>

Galileo's programme of materializing geometry assumes that the forms of geometrical figures are perfectly realized in material bodies. Errors would therefore lie, as Salviati - Galileo's mouthpiece in the Dialogue - maintains,

'not in the abstractness or concreteness, not in geometry or physics, but in a calculator who does not know how to make a true accounting. Hence, if you had a perfect sphere and a perfect plane, even though they were material, you would have no doubt,' Salviati assures Simplicius, the Peripatetic interlocutor - 'that they touched in one point.'<sup>3</sup>

Kepler however was too well aware of the gap between geometrical forms and their embodiments. Had he participated in this dialogue, he probably would not have accepted Salviati's assurance of the material existence of perfect geometrical forms. Indeed, as Caspar points out in his biography of Kepler, he was 'always worried by the fact that the insertion of the regular solids does not present the distances of the planets exactly'.<sup>4</sup> These discrepancies had haunted him for many years until he came to realize, in his work on the concept of harmony, that they must be there. 'The geometric cosmos of a perfect insertion had no place,' Kepler maintains, 'next to the other cosmos which was the most

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1. Mach, 1974, p.309.

2. Russell, 1975, p.743.

3. Gallileo, 1974, pp.207-8. Galileo appears to treat errors in the same idealized fashion in which he treats physics at large; that is, he implies that errors can be exactly calculated. It should be however noted that he does recognize various types of error, notably errors of observation and instrumental errors. (Ibid., pp.289-93, 296, 301, 387-88.)

4. Caspar, 1959, p.287.



harmonic possible.'<sup>1</sup> In order that the harmonies could be expressed in the motions, Caspar explains Kepler's position, 'the values of the distances supplied by the regular solids had to undergo little changes.'<sup>2</sup> Discrepancies are thus recognized as an essential feature of the unity of matter and form.

A closer examination of Kepler's work is now befitting and I turn to his study of the motion of Mars which constitutes the Astronomia Nova.<sup>3</sup> It is in the course of this study that Kepler writes: 'Now because they could not be dismissed, these eight minutes alone, therefore, led the way to the reformation of the whole of Astronomy, and are made the subject-matter for a large part of this work.'<sup>4</sup>

Although Copernicus is popularly believed to inaugurate the scientific revolution, his work, compared closely with that of Ptolemy, does not admit the epitaph 'revolutionary'. After the first section of De revolutionibus (1543) which advances philosophical arguments for the mobility of the earth, Copernicus begins to expound the technical mathematics of his system. At this point, as D.J. de Solla Price points out in a paper entitled 'Contra-Copernicus', the book becomes 'little more than a re-shuffled version of the Almagest'.<sup>5</sup> Price concedes, however, that Copernicus had the best of reasons<sup>S</sup> for wishing his new ideas to appear clothed in the respectable and conservative form of traditional astronomy. Thus, Copernicus' magnum opus duplicates not only the mathematical machinery of Ptolemy but also the method and structure of his book. 'Chapter by

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1. Quoted by Caspar, *ibid.*, p.288.

2. *Ibid.*, pp.287-8. Cf., Koyré, 1973, p.284. Koyré maintains that Kepler treated his scheme as a mere approximation which he subordinated to a higher structure of numbers and harmony. In my view Kepler gave both matter and form equal weight.

3. The following exposition relies on the works of Dreyer, Koyré and Curtis Wilson.

4. Quoted by Koyré, 1973, p.401 note no.22.

5. Price, 1962, p.215; for another view, see Koyré, 1973, p.111 note no.8.

chapter', Price continues to compare these two celebrated works, De revolutionibus

'has the same format and language, the same arrangement of subject-matter with only the slightest changes as dictated by the change to heliocentricity... The Almagest is not an easy book to read, but it was at least original in many of its parts. The magnum opus of Copernicus does not have that distinction beyond its first few pages. It contains a few new observations and computations based on them, but beyond that it paraphrases Ptolemy, putting his work into a somewhat transparent new cloak.'<sup>1</sup>

Price's strong view seems to be an amplification of Neugebauer's concluding remark in the first appendix to his book, The Exact Sciences in Antiquity, where he analyses the Ptolemaic system and its Copernican modification.

'There is no better way to convince oneself of the inner coherence of ancient and mediaeval astronomy,' Neugebauer observes, 'than to place side by side the Almagest, al-Battani's Opus astronomicum and Copernicus' De revolutionibus. Chapter by chapter,<sup>2</sup> theorem by theorem, table by table, these works run parallel.'

In Neugebauer's view the spell of tradition was broken with the words of Tycho Brahe and Kepler. 'The very style in which these men write is totally different from the classical prototype.' And Neugebauer concludes that 'never has a more significant title been given to an astronomical work than to Kepler's book on Mars: "Astronomia Nova".'<sup>3</sup>

Astronomia Nova is indeed a new astronomy. Its very title, as Koyré remarks, 'proclaims, rather than foretells, a revolution;'<sup>4</sup> it reads: 'A New Astronomy BASED ON CAUSES, or Celestial Physics, propounded by commentaries on the motion of the star Mars, from observations of Tycho Brahe.'<sup>5</sup>

1. Price, *ibid.*

2. Neugebauer, 1969, pp.205-6.

3. *Ibid.*, p.206.

4. Koyré, 1973, p.166.

5. ASTRONOMIA NOVA AITIOAOTHTOΣ, seu PHYSICA COELESTIS, tradita commentariis de motibus stellae MARTIS, Ex observationibus G.V. TYCHONIS BRAHE; see the title-page in Beer and Beer, 1975, p.76.

Moreover, as Neugebauer rightly points out, the style of writing which Kepler had adopted is, like the astronomy he developed, completely new. In fact, the Astronomia Nova is for the most part a diary of Kepler's ideas; as he explains in the preface, he is not merely interested to impart to the reader what he has to say but, as he puts it,

'above all to convey to him the reasons, subterfuges, and lucky hazards which led me to my discoveries. When Christopher Columbus, Magelhaen, and the Portuguese relate how they went astray on their journeys,' Kepler continues, 'we not only forgive them, but would regret missing their narration because without it the whole, grand entertainment would be lost. Hence I shall not be blamed if, prompted by the same affection for the reader, I follow the same method.'<sup>1</sup>

More specifically, Kepler states that it is his intention to promote astronomical doctrine in such a way that 'one can compute by means of the tables corresponding to the celestial phenomena, that which it has not been possible to do hitherto with sufficient 'accuracy'.<sup>2</sup> And he gives the following examples:

in August 1608 the planet Mars was almost 4 degrees away from the position assigned to it by the Prussian computation [that of E. Reinhold]. In August or September 1593, this error, which has been completely eliminated in my computations, amounted to nearly 5 degrees.<sup>3</sup>

These remarks indicate, I suggest, that Kepler was indeed interested in errors as a means of attaining knowledge: he acknowledged errors, and the attempt to eliminate them placed him in a path of discovery.

The choice of Mars as an object of inquiry was of course vital to

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1. Quoted by Koestler, 1961, p.124; Kepler, 1938-1975, III p.36. Faraday's diaries constitute another example. 'Comparing the methods of Ampère and Faraday, Maxwell warned his students that it was necessary to study both in order to get a view in depth of a scientific theory. Ampère... does not show the steps by which he arrived at his perfect demonstration: "He removed all traces of the scaffolding by which he had raised it." Faraday... made known both his successful and his unsuccessful experiments, both his crude and his developed ideas... if Ampère's research should be read... as a "splendid example of scientific research", Faraday's writings should be studied "for the cultivation of a scientific spirit".' (Jaki, 1970, pp.519-20.)
  2. Quoted by Koyré, 1973, p.167.
  3. Ibid.

Kepler's eventual success. The orbit of Mars deviates considerably from the circle: it is most pronouncedly elliptical, and there is no wonder therefore why it defied Tycho's attempt to determine its elements. Tycho could not reconcile his theory with the observations and invited Kepler to assist in this attempt.<sup>1</sup>

In the dedication of the Astronomia Nova to the Emperor Rudolph II, Kepler allegorically declares war on Mars; Mars, he says,

is the mighty victor over human inquisitiveness, who made a mockery of all the stratagem of astronomers...thus did he keep the secret of his rule safe throughout all past centuries and pursued his course in unrestrained freedom; wherefore that most famous of Latins, the priest of nature Pliny,<sup>2</sup> specially indicted him: Mars is a Star who Defies Observation.

Characteristically, Mars had been taken to defy observation, not theory; a view which Kepler was intent on rectifying in his new strategy.

Whether or not Kepler's declaration of war on Mars has to do with a tradition that sees nature as begotten from war, is difficult to establish. However, since Pico della Mirandola influenced, in my view, Kepler's way of thinking and since Kepler himself aspired to theological understanding, it is worth quoting Pico on the wars of the spirit. In his Oration on the Dignity of Man, Pico assigns to natural philosophy the task of allaying

'the strife and differences of opinion which vex, distract, and wound the spirit from all sides. But,' Pico continues, 'she will so assuage them as to compel us to remember that, according to Heraclitus, nature was begotten from war, that it was on this account repeatedly called "strife" by Homer, and that it is not, therefore, in the power of natural philosophy to give us in nature a true quiet and unshaken peace but that this is the function and privilege of her mistress, that is, of holiest theology.'<sup>3</sup>

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1. Cf. Gingerich, 1975, p.264.

2. Quoted by Koestler, 1961, p.125. Kepler's words are: 'Hic est ille potentissimus inventionum humanarum Domitor: qui omnibus Astronomorum irrisu expeditionibus, elisi machinis, profligatis copiis hostilibus, Secretum Imperii sui, cunctis retro saeculis custoditum possederat securus, cursusque suos exercuerat liberrimus et incircumscriptus: ut praecipuam querelam instituerit Mystes ille Naturae, Latinorum celeberrimus C. Plinius: Martis inobservabile sidus esse.' (Kepler, 1938-1975, III p.8.)

3. Pico, 1948, p.231.

Thus, in Pico's view, natural philosophy in general can not bestow peace (the reader may remember Newton's 'never-at-rest' predicament), and, in particular, Tycho's natural philosophy of Mars did not leave Kepler in 'a true quiet and unshaken peace'.

At the centre of Kepler's strategy stands the concept of physical causes; a concept which he had outlined in his youthful work Mysterium Cosmographicum, and brought into fruition in Astronomia Nova. It maintains that physical causes can have their origin only in a real, physical body; they cannot emanate from a mathematical point, where there is nothing at all.<sup>1</sup> Thus,

'the first step towards determining the physical causes [of planetary motion] consists,' according to Kepler, 'in proving that the common point of the eccentrics [that is to say, the point to which the motions of the eccentric planetary orbits must be referred] is not some point or other in the vicinity of the Sun, as believed by Copernicys and Tycho Brahe, but is the centre of the solar body itself.'<sup>2</sup>

Kepler shifted therefore the common point of the planets' orbits to the 'body of the Sun', displacing thereby the line of apsides and the positions of the planets when at opposition; a shift which, in the case of Mars, amounted to 5 degrees in longitude - a difference of the same order as the afore-mentioned errors.<sup>3</sup>

Kepler's war on Mars can be divided into seven phases.<sup>4</sup> Having realized that Tycho's theory of Mars failed completely to give proper values for the latitude of Mars' oppositions with the sun, Kepler decided to embark upon the war with an attack on the problem these discrepancies had presented. I may add in passing that Tycho's predictions for the

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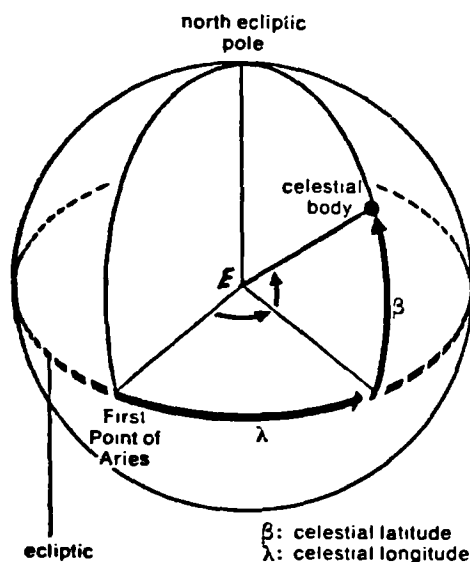
1. Koyré, 1973, p.168.

2. Ibid.; in the Epitome, Kepler writes: 'The seat to be assigned to this same source of movement is not in any mathematical point, very near to the most noble body, but rather in that most noble body [namely, the sun]...' (Kepler, 1952, p.910.)

3. Koyré, *ibid.*

4. Wilson, 1972, p.94.

longitude of these oppositions were, however, fairly accurate: the difference being not more than 2 minutes, that is, the claimed accuracy of Tycho's observations. Thus the first phase of the war is concerned with the latitude of Mars.<sup>1</sup>



The ecliptic system

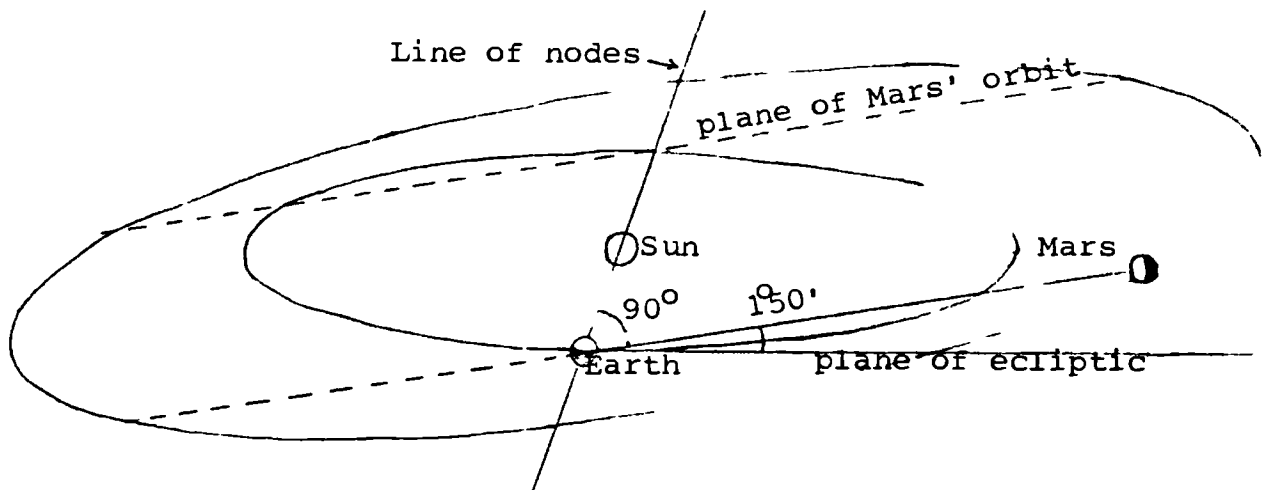
In accordance with his belief in physical causes, Kepler hypothesized that the plane of Mars' orbit is inclined at a constant angle to the plane of the ecliptic and passes through the body of the sun.<sup>2</sup> In contrast, Copernicus, like Ptolemy, had postulated that the orbital plane of Mars oscillates in space and, furthermore, made the oscillations depend upon the earth's position - a 'monstrous' idea in Kepler's view.<sup>3</sup> Kepler then proceeded to determine the inclination by three different methods. Two of these methods assume the ratio of the dimensions of the orbits to be known and thus are dependent upon theory; Kepler resorted therefore to another method which is quite independent of any previous theory.

1. Koyré, 1973, pp.172-3.

2. Wilson, op.cit., p.94.

3. Koestler, 1961, p.127. Kepler's words are: 'Imposuit Ptolemaeo hypotheseos suae perlexitas, ut monstra multa congererit in doctriam latitudinum.' (Kepler, 1938-1975, III p.140.)

It consists in observing Mars at a time when it is in quadrature to the sun, while the earth and the sun are both in the line of nodes, that is, the line on which the plane of Mars' orbit cuts the plane of the earth's orbit - the ecliptic. At this juncture the observed angular distance of Mars from the ecliptic - its latitude - would be equal to the angle between the planet's orbital plane and the ecliptic, that is, its inclination.<sup>1</sup>



Using Tycho's observations, Kepler established the constancy of the inclination and found it to be one degree and 50 minutes. He thus demonstrated that the shift of the common point of the planets' orbital planes into the true sun provides a constant value for the inclination, and proved that its apparent variation is the result of making the orbital plane pass, not through the true sun, but either through the earth (in Ptolemy's theory), or through the centre of its circle (in Copernicus' theory), or - what amounts to the same thing - through the mean sun (in Tycho's theory).<sup>2</sup> This was Kepler's first victory; here, for once, he

1. Dreyer, 1953, pp.382-83; cf., Wilson, op.cit.

2. Koyré, 1973, p.172.

met with immediate success and quite confidently remarked: 'The observations took the side of my preconceived ideas as they had done before.'<sup>1</sup>

Clearly, the use of three different methods, of which one is independent of the cosmological theories at issue, to establish the phenomenon of constant inclination, provided Kepler with a sound ground from which to pursue his studies further. Independent methods which arrive at the same result furnish the strongest proof for its correctness; Kepler shows here, as he will do later, a distinct insight into the scientific method.<sup>2</sup>

Having established the constancy of the inclination, Kepler proceeded to examine the longitude problem of Mars and to develop its theory. In this phase of his war, the second one, Kepler incurred a defeat but gained ground all the same through his determination to understand the significance of the error which lay at the centre of that defeat. Indeed, the acknowledgment of this error ushered in a new era in astronomy.

The fundamental element of this phase is Kepler's return to a Ptolemaic concept of orbital motion. Whereas Copernicus had dispensed with Ptolemy's equant - the point, at some distance from the centre, from which the planet appears to have uniform motion - and reverted, so to speak, to Hipparchus in order to maintain the principle of uniform circular planetary motion in all its rigour; Kepler reached back to Ptolemy, renounced the dogma of uniform motion and revived the equant as a mathematical means for probing the circular orbit.<sup>3</sup> Much of the simplicity which Copernicus had gained through setting the earth into orbital motion, was lost as a result of his insistence upon uniform orbital motion; an assumption which requires more epicycles 'to save the phenomena' than that of Ptolemy.<sup>4</sup> However,

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1. Quoted by Koestler, *op.cit.*, p.128; Kepler, 1938-1975, III p.141.

2. Cf. Wilson, 1969, p.94; *supra*, p.69.

3. Koyré, 1973, p.176.

4. Price, 1962, pp.206-7, 214-15.



it was not only simplicity and elegance that guided Kepler; his strong belief in physical causes would not extend to an artificial device such as the superposition of uniform circular motions, which was designed to account for the observed phenomenon of variation in speed of planetary motions. To be sure, the Ptolemaic concept of equant does not in itself explain, let alone physically elucidate, this phenomenon, but at least it suggests a direction for further investigation; that is, it raises a new question which Kepler recognized as more fruitful than the epicyclic answer. In anticipation of Kepler's ideas I may note here that the physical explanation which Kepler suggested for the variation in speed constitutes the third phase of his war.

But to return to the second phase; having opted for a Ptolemaic concept of orbital motion, Kepler proceeded to determine afresh the elements of Mars' orbit by referring it to the true body of the sun and assuming a physical non-uniform motion on its circle. For that purpose he had to determine the position of the line of apsides (longitude of the aphelion), the eccentricity and the mean anomaly (the time - expressed in degrees - taken by the planet to describe a section of its orbit starting from the apside) for any given date. Looking for the determination of these elements, Ptolemy, who had bisected the eccentricity, and Copernicus and Tycho, who had made the equant point coincide with the centre of the circle, could satisfy themselves with the data provided by observing the position of Mars at three oppositions. Kepler, in contrast, sought to place the equant in such a way that the best match between observations and theory would be obtained, and he chose to determine its position on purely empirical grounds. He resorted to the data of four oppositions, taking the fourth as a means of testing.<sup>1</sup>

An opposition occurs about every 780 days when Mars, earth and sun

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1. Wilson, 1972, p.96; Dreyer, 1953, p.383; Koyré, 1973, p.173.

align to form the line M-E-S; at this juncture Mars can be observed approximately on the meridian overhead at midnight.<sup>1</sup> At the exact time of opposition Mars is seen from earth against the background of the stars in just the position it is seen from the sun. However, it is not a trivial task to determine an opposition, as its exact moment, and hence position, cannot be perceived distinctly. More specifically, the points of the zodiac, which a planet occupies in oppositions, are not, as in lunar eclipses, distinguished by any sensible marks, but must be determined by calculation from a group of observations coupled with the knowledge of the mean solar motions.<sup>2</sup> Such calculations - a practice which the ancients had established - resulted therefore not in the knowledge of true oppositions but rather in information about their means. Copernicus, who followed this practice, calculated - when he observed a planet at about the time of an expected opposition - the mean place of the sun known by its mean motion, and compared it with the observed place of the planet: if the position of the observed planet were to differ by exactly  $180^\circ$  from the mean place of the sun, he would then contend that a mean opposition had been obtained; otherwise, he would attempt, from a comparison of the motions of the planet observed for several nights successively, to determine the instant when the exact difference of  $180^\circ$  took place.<sup>3</sup> Kepler, who, as I have noted, had done away with the concept of the mean sun, categorically objected to such a practice. He clearly saw that the lack of a physical solar theory had put astronomers - both ancients and contemporaries - in this inevitable position of referring to an abstract alignment rather than to a physical one. It seemed vital to him, therefore, to find corrections for all the mean oppositions, except of course for those observed in the line of the solar apsides. Without such corrections it was vain, he believed, to expect

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1. Wilson, *ibid.*, p.95.

2. Small, 1963, p.149.

3. *Ibid.*, pp.149-50.

more from any theory, however sound its principles might be.<sup>1</sup> He thus chose from the ten sets of the opposition observations of Tycho (to which he added two of his own, in 1602 and 1604), four sets which are reasonably uniformly distributed on the orbit, and proceeded to interpolate from them the times for the true oppositions; that in turn provided him with the data of the true heliocentric longitude of Mars at these four positions.<sup>2</sup>

The next step in this phase of the war on Mars was the attainment of the solution for the following geometrical problem: out of the four sets of data to determine (1) the radius of the orbit; (2) the direction of the line formed by the three points: the centre of Mars' orbit, the location of the sun and the equant point; and (3) the ratios of the distances between these points to the radius of Mars' orbit. As Koestler remarks,

it was a problem which could not be solved by rigorous mathematics, only by approximation - that is, by a kind of trial-and-error procedure which has to be continued until all the pieces in the jigsaw puzzle fit together tolerably well.<sup>3</sup>

The problem Kepler set himself could be solved, in other words, only iteratively. Concerning this complicated procedure, Kepler implored his reader to take pity on him.

'If you are wearied by this tedious method,' he entreats the reader, 'take pity on me, who carried out at least seventy trials of it, with the loss of much time, and don't be surprised that this already is the fifth year since I have attacked Mars, although the year 1603 was almost entirely spent on optical investigations.'<sup>4</sup>

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1. Ibid., pp.150-154. According to Small the reference to the centre of the sun 'was an improvement more important, and of greater consequence, to simplify the science, than any [excluding only the system of Copernicus] which had been introduced in all the preceding ages; and his successful and decisive establishment of its truth and propriety, may be justly ranked among his greatest discoveries; and equally deserves our attention with those which have been more generally celebrated'. (p.154.)
  2. Dreyer, 1953, p.383.
  3. Koestler, 1961, p.131.
  4. Quoted by Gingerich, 1964, p.218.

As Gingerich reports, it took him eight days to programme this very procedure, and altogether another 12.4 minutes of IBM-7094 time.<sup>1</sup> Gingerich notes further that

'instead of requiring seventy trials as Kepler did, the computer program, using identical methods, took only nine trials!....Why, then,' asks Gingerich, 'did Kepler require seventy trials?'<sup>2</sup> And he tentatively answers that 'Kepler was horribly plagued by numerical errors, that his trials accidentally diverged nearly as often as they converged.'<sup>3</sup>

Seven years later, having examined the Leningrad Kepler manuscripts, Gingerich produced an alternative hypothesis for Kepler's 70 iterations.

'Since he eventually had observations for 12 Martian oppositions, possibly,' Gingerich suggests, 'Kepler carried out the procedure repeatedly for different sets of four oppositions. If so, this would have been an early attempt to control the errors of observational data, and hence exceedingly interesting to the history of astronomy.'<sup>4</sup>

One might also say to the history of science. Gingerich arrived at the conclusion that Kepler had always worked with the same four oppositions, but that he repeatedly tested the results against additional oppositions.<sup>5</sup>

Notwithstanding the enormous difficulties - for the resolution of which Kepler had unsuccessfully appealed for help to Moestlin, Magini and Vieta - he did eventually emerge victorious, or so he thought. Kepler thus proclaimed another victory over Mars: 'Thou seest now,' he addresses the reader, 'that the hypothesis based on this method not only satisfies the four positions on which it was based, but also correctly represents, within two minutes, all the other observations.'<sup>6</sup> However, having established his theory, Kepler exclaims: 'Who would have thought it possible? This hypothesis, which so closely agrees with the observed oppositions,

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1. Ibid., pp.221-22.

2. Ibid., p.223.

3. Ibid., p.224.

4. Gingerich, 1973, p.309.

5. Ibid., p.314.

6. Quoted by Koestler, 1961, p.133.

is nevertheless false.'<sup>1</sup>

Much to his dismay, Kepler discovered that although the theory he had established was accurate in predicting heliocentric longitudes, it failed completely in another respect. The concordance with the available observations of the longitudes of Mars was sufficiently good; indeed, it was quite impressive: the largest discrepancy did not exceed 2 minutes and 12 seconds – its average being 50 seconds – a discrepancy which Kepler held to be chiefly due to errors of observation.<sup>2</sup> The theory failed, however, in its account of the distances of Mars from the sun. Kepler came to call it "hypothesis vicaria": the vicarious theory, since although it was false it did yield the correct heliocentric longitudes of Mars. The failure of this theory, or rather the comprehension of its failure, is the root of Kepler's victory over Mars.

How then did Kepler come to realize that his successful theory was in fact wrong? The answer, I suggest, is twofold: it has both epistemological and methodological aspects. From the epistemological point of view, Kepler wished – the reader may recall his letter to Fabricius in 1608 – to weld his newly found theory with the physics of the Copernican system as he had conceived of it; that is, Kepler was not satisfied with a theory which establishes only angular distances between the phenomena. Computation of an orbit which is taken to represent a really physical phenomenon, presupposes a knowledge of the linear distances of the planet from the sun; such knowledge, as Koyré points out, 'ancient astronomy did not, and could not, have. We could go even further and say,' Koyré holds,

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1. Quoted by Koestler, 1961, p.133. Kepler's words are: 'Fieri quis posse putaret? Haec hypothesis observationibus ἀκροβυτίαις tam prope consentiens falsa tamen est, sive observationes ad medium Solis locum sive ad apparentem examinentur.' (Kepler, 1938-1975, III p.174.)

2. Wilson, 1972, p.96; Dreyer, 1953, p.385.

'that the orbit of a planet had no real existence for pre-Copernican astronomy. It interested no-one,' he concludes.<sup>1</sup> For Kepler, in contradistinction, the study of the orbit as a physical phenomenon, and hence the determination of the distances of a planet from the sun, was of prime importance; indeed, the Astronomia Nova is the culmination of such a study. Furthermore, methodologically Kepler wanted to test, as he had done in the first phase of the war, whether or not the new theory could withstand independent examination; that is, he presumably asked himself the question as to whether or not the new theory is compatible with an established theory, e.g., Tycho's solar theory transformed into a theory of the earth's motion?

From his optical studies of the apparent diameter of the sun's disk - using an instrument which he had invented for that particular purpose, and having actually observed that the diameter in question does not change considerably (about one minute, the diameter itself being 30 minutes)<sup>2</sup> - Kepler concluded that the distance between the earth and the sun does not change very much (the mean sun, not the sun itself, is at the centre of the earth's circular orbit in Tycho's transformed solar theory). In fact, it changes less than Tycho's theory predicts. Hence the orbit must be nearly circular. Kepler thus felt confident enough to apply a transformed Tychonian solar theory in the attempt to determine the distance Mars-sun.<sup>3</sup>

He selected two observations of Mars when it is not in opposition. In this position the planetary bodies are not aligned and thus form a triangle M-E-S. Two triangles were obtained which could be solved trigonometrically by using (1) the data of Tycho's direct observations (earth-Mars positions);

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1. Koyré, 1973, p.419 note no.6; cf., pp.238, 403 note no.1. Koyré generalizes too swiftly; Ptolemy, for one, did show interest in linear distances. (Supra, pp.69-70.) Nevertheless, as a characterization of a general attitude this remark of Koyré is correct.
  2. Linnik, 1975, pp.813-14; Chenakal, 1975, p.821; Dreyer, 1953, p.387; Wilson, 1968, p.7.
  3. Wilson, 1972, p.96.

(2) the results of the vicarious theory (sun-Mars positions); and (3) the results of the transformed Tycho's solar theory (sun-earth positions and distances). Kepler could thus determine the two sun-Mars distances.<sup>1</sup>

The computation of the sun-Mars distances close to the line of apsides, showed that the centre of Mars' orbit must be located half-way between the sun and the equant point; in other words, this new calculation vindicated Ptolemy's assumption of the bisection of the eccentricity. The vicarious hypothesis, on the other hand, had placed the centre of Mars' orbit some six-tenths of the eccentricity away from the sun.<sup>2</sup> The attempt to bisect the eccentricity in the context of the vicarious theory resulted, Kepler found out, in quite a satisfactory agreement between observed and calculated positions near the apsides and positions orthogonal to them; however, at intermediate positions, in anomalies 45°, 135°, etc., the discrepancy between calculation and observation amounted to 8 minutes.

'We see now,' Kepler explains, 'why Ptolemy acquiesced in the bisection of the eccentricity, for 8' was well within the limit of accuracy to his observations(10');<sup>3</sup> however, Kepler continues, 'seeing that the Divine Goodness has given us in Tycho Brahe a most diligent observer whose observations have revealed the error of 8' in Ptolemy's calculation, it is only right that we should thankfully accept this gift from God, and put it to good use. We must undertake to discover ultimately the true nature of celestial motions.'<sup>4</sup>

And he declares that

'henceforth I shall lead the way toward that goal according to my own ideas. For, if I had believed that we could ignore these eight minutes, I would have patched up my hypothesis accordingly. But since it was not permissible to ignore them, those eight minutes point the

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1. Ibid; Koyré, 1973, p.178; Dreyer, 1953, p.385.

2. Wilson, *ibid*.

3. Quoted by Dreyer, *op. cit*.

4. Quoted by Koyré, 1973, p.178.

road to a complete reformation of astronomy: they have become the building material for a large part of this work.'<sup>1</sup>

The central question that has to be considered is of course why was it not permissible to ignore the 8' discrepancy. Kepler, I suggest, did not do full justice to his cause when he explicitly referred, with much reverence, solely to Tycho. To be sure, Tycho's improvements in observational technique and the substantial increase - the result of a painstaking effort - in the accuracy of the observations Tycho had himself carried out, changed indeed, as Koyré remarks, 'the whole situation: a difference of 8' compared with Tycho Brahe's data could not be ignored'.<sup>2</sup> Moreover, Tycho had also improved the accuracy of the planetary parameters and thus created, in Price's words, 'for the first time in history', the opportunity to detect a clear discrepancy between the observed place of a planet and that predicted by a theory.<sup>3</sup> Kepler encountered such an opportunity and made, no doubt,

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1. Quoted by Koestler, 1961, p.134. This important passage deserves a full quotation from the original: 'Atque ex hac tam parva differentia octo minutorum patet causa, cur Ptolemaevs, cum bisectione opus habuerit, acquieverit puncto aequatorio stabili. Nam si aequantis eccentricitas, quantam indubie poscunt aequationes maximae circa longitudes medias, bisectur, vides omnium maximum errorem ab observatione contingere VIII minutorum, idque in Marte, cujus est eccentricitas maxima; minorem igitur in caeteris. Ptolemaevs vero profitetur, se infra X minuta seu sextam partem gradus observando non descendere. Superat igitur observationum incertitudo seu (ut ajunt) latitudo hujus calculi Ptolemaici errorem.

'Nobis cum divina benignitas Tychonem Brahe observatorem diligentissimum concesserit, cujus ex observatis error huius calculi Ptolemaici VIII minutorum in Marte argitur; aequum est, ut grata mente hoc Dei beneficium et agnoscamus et excolamus. In id nempe elaboremus, ut genuinam formam motuum coelestium (his argumentis fallatcum suppositionum deprehensarum suffulti) tandem indagemus. Quam viam in sequentibus ipse pro meo modulo allis praeibo. Nam si contemnenda censuissem 8 minuta longitudinis, jam satis correxissem (bisecta scilicet eccentricitate) hypothesim cap. XVI inventam. Nunc quia contemni non potuerunt, sola igitur haec octo minuta viam praeiverunt ad totam Astronomiam reformandam, suntque materia magnae parti hujus operis facta.' (Kepler, 1938-1975, III pp.177-78; cf., Koyré, 1973, p.401 note no.22.)

2. Koyré, 1973, pp.178-9.

3. Price, 1962, p.211.



the best out of it. But at the root of this exercise lies, I maintain, his belief in physical causes and the reality of objects. Here I am in agreement with Koestler who holds that it was Kepler's

'introduction of physical causality into the formal geometry of the skies which made it impossible for him to ignore the eight minutes arc. So long as cosmology was guided by purely geometrical rules of the game, regardless of physical causes,' Koestler maintains, 'discrepancies between theory and fact could be overcome by inserting another wheel into the system. In a universe moved by real, physical forces, this was no longer possible. The revolution that freed thought from the stranglehold of ancient dogma immediately created,' Koestler concludes, 'its own rigorous discipline.'<sup>1</sup>

The results of the studies in the second phase showed Kepler that the two Ptolemaic principles, namely the circularity of the orbit and the uniformity of the motion about an equant point – the principles upon which the vicarious theory rested – were questionable. He realized, in fact, that one or the other or both must be wrong.<sup>2</sup> The distance determination showed him that Mars was nearer to the sun than his theory required; that is, its orbit was curved within the hypothetical circular orbit. If, however, the orbit were to remain circular, the equant point would not then be fixed and would have to oscillate backwards and forwards along the line of apsides; a possibility which, in Kepler's view, could not have been the effect of any physical cause.<sup>3</sup>

The setback that Kepler had suffered by the failure of the vicarious theory, proved later on to be a great asset. It convinced him that a correct

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1. Koestler, 1961, p.135; cf., Koyré, 1973, pp.221, 227.

2. Wilson, 1972, pp.96-99.

3. Dreyer, 1953, pp.385-6; cf., Koyré, 1973, p.179. Neugebauer draws a characteristic contrast between Ptolemy and Kepler concerning their attitudes towards discrepancies. 'The fact that Ptolemy's new lunar theory was highly successful in the prediction of lunar longitudes is the obvious reason for disregarding its glaring inadequacy to account for the smallness of the variations in apparent diameter and lunar parallax. The situation has a certain parallel,' Neugebauer remarks, 'in the development of Kepler's theory of Mars when he used one model to correctly predict the longitudes of the planet, another for its distances. Kepler did not rest until he was able to reconcile both aspects of the problem whereas Ptolemy's theory was accepted for centuries without any attempt to eliminate its deficiencies.' (Neugebauer, 1975, p.98.) Cf., supra, p.74.

theory of Mars could not be formed from oppositions alone. He therefore embarked, in the third phase of the war, upon a more general theory involving the earth as well. This phase culminates in the emergence of a new rule - the distance rule - which Kepler designed as a vehicle for his quest to obtain physical interpretation amenable to mathematical manipulation.<sup>1</sup>

Since Kepler obtained a verification for the bisection of the eccentricity of the orbit of Mars, and, furthermore, since Ptolemy, quite understandably, did not apply this concept to the orbit of the earth (he had applied it though in his theories of Venus, Mars, Jupiter and Saturn), Kepler decided to extend this concept to the earth's orbit.<sup>2</sup> Bisecting the eccentricity of the earth's orbit meant that an explanation for the apparent change of the diameter of the annual earth's orbit was immediately forthcoming: the phenomenon would originate then in the separation between the centre of equal distances and the centre of equal angular motions; the two centres were not coincident just as they were not coincident in any other planetary orbits. Such an extension suited Kepler very well; it was in line with his unifying approach towards the planetary system.<sup>3</sup>

To test this contention, Kepler resorted to an ingenious and original procedure: he examined the earth's orbit from the standpoint, as it were, of Mars; that is, he imagined himself observing the earth from Mars. To simplify the calculations, he, so to speak, immobilized Mars by choosing those observations which were taken when Mars, having completed its sidereal period (687 days), returned to the same place on its orbit.

Since a circle is determined by three points, Kepler chose three observations and obtained another one, as he invariably did, to serve as a check. The data obtained from Tycho's observations determined the three directions earth-Mars; Tycho's solar theory determined the three directions sun-earth

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1. Dreyer, *ibid.*, p.386.

2. *Ibid.*; cf., Wilson, 1972, p.99.

3. Dreyer, *ibid.*

and the vicarious theory determined the single direction sun-Mars. Observations and calculations thus yielded three triangles with all the angles known and one side - sun-Mars - in common. By taking the ratios of the lengths of each of the sun-earth lines to the length of the single sun-Mars line, and hence to each other, Kepler was able to determine the position of the earth in its orbit at each of the three observations, and hence the dimension of the earth's circular orbit. That in turn made it possible to determine the position of the orbit's centre and, in relation to the radius of the orbit, its distance from the sun.<sup>1</sup> The results of these trigonometrical calculations vindicated Kepler's belief that the eccentricity of the earth's orbit ought to be bisected and, as a consequence, that the earth moves according to the same principles upon which the planets' motions rest. Furthermore, the small value of the eccentricity agreed with the very small variation in the apparent diameter of the sun's disk in the course of a year.<sup>2</sup>

Although the results were very satisfying to Kepler - they proved him right with regard to the structural uniformity of the planetary universe - he did not rely upon this single proof but, faithful to his methodology, proceeded to execute two other independent tests which confirmed these important results.<sup>3</sup> These independent tests were all the more important since, as Wilson points out,

into the solution of the three triangles went seven pieces of data, each of which could be in error. The trigonometric calculation could greatly magnify the initial errors, particularly where small angles were involved. Kepler went through procedures of this kind with several sets of observations, and he got divergent results; the largest result for the distance from the sun to the centre of the circular orbit was two thirds ... larger than the smallest result.<sup>4</sup>

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1. Koyré, 1973, pp.181-2; Wilson, 1968, pp.5-6; Wilson, 1972, p.99.

2. Dreyer, 1953, p.387.

3. Ibid., p.386; Koyré, 1973, pp.182-3.

4. Wilson, 1972, pp.99-100.

It should be further noted that all of the actual distances that Kepler found are wrong. This is due to the fact that the distances were determined as a function of the distance sun-earth which is twenty times greater than the value Kepler accepted. However, when the calculations are consistently carried out with relative values, the incorrect data can furnish correct quantitative results.<sup>1</sup> Nevertheless, the erroneous values of the distances constitute epistemologically a defect since they undermine Kepler's claim to conceive of the planetary system as a physical phenomenon, for which it is imperative to get the actual dimensions right.

But to return to the latter part of phase three. Having proven that the eccentricity of the earth's orbit should be, like the eccentricities of the other planets' orbits, bisected - which means that the earth's motion is truly non-uniform - Kepler proceeded to inquire into the causes and structure of planetary motion and did not renew, as one would have expected, his direct attack on Mars.<sup>2</sup>

The confirmation of the truly non-uniform motion of the earth suggested to Kepler that he should revive his conjecture on the planetary motion, which he had put forward in his youthful Mysterium Cosmographicum. He had there contended that a planet moves fastest at the perihelion and slowest at the aphelion, since at these extreme positions the planet is placed nearest to and furthest from the sun, and thus bears respectively most and least of the influence of some power emanating from the sun.<sup>3</sup> Koyré suggests that the striking success in referring the non-uniform motion of the planets to the true sun instead of to an abstract point like the mean sun, might have increased Kepler's aversion to using the equant point - another abstract point - to describe the planetary motions. It

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1. Koyré, 1973, pp.107 note no.24, 403 note no.7.

2. Ibid., p.184.

3. Dreyer, 1953, p.386.

might have also increased his desire, Koyré suggests further,

to give a mathematical form to the orderly rule of behaviour in his system - planets move slower the farther they are away from the Sun. This mathematical form would give direct expression to the dynamic structure of the motion in question, instead of employing an artificial "trick" such as the equant, and consequently would enable its nature to be understood, in addition to allowing calculations to be performed.

Kepler, it seems, found it necessary to develop a completely new physics - physica coelestis, as he called it - with its own mathematical technique, based not on abstract concepts such as the mean sun and the equant point which are in effect mathematical fictions, but on real entities whose structures must be faithfully reproduced.

Kepler regarded in his physica coelestis the concept of distance in relation to the degree of motion. 'A greater or less distance will result in a greater or shorter time for traversing the path,'<sup>2</sup> he argues. Distance, he points out, relates to objects placed in relation to one another; hence, 'the cause of the changes in motion must be found', Kepler maintains, 'in one of the relata....Therefore, the only hypothesis that we can assume is the one which places the cause of the increase and decrease [in motion] in the origin of the relata, namely, in the point which we have taken as the centre of the Universe and from which the distances have been calculated. Consequently', Kepler continues to argue, 'if as a result of an increase, or decrease, in the distance of the body of the planet from the centre of the Universe the motion of the planet become slower, or faster, respectively, [then] the source of the motive force must necessarily be in the point which we have taken as the centre of the Universe.'<sup>3</sup>

In Kepler's view, 'it was more likely for this point...to be occupied by the sun, or the earth in Ptolemy's view, than by nothing at all'.<sup>4</sup>

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1. Koyré, 1973, pp.183-84.

2. Quoted by Koyré, 1973, pp.188-89; cf., pp.238-39.

3. Quoted by Koyré, *ibid.*, pp.189-90.

4. Quoted by Koyré, *ibid.*, p.190.

And proving, as Kepler had done,

'by means of observations, that we must necessarily refer the planet Mars to positions of the visible [true] sun,' it became apparent to him that 'the sun is situated at the centre of the system,...[and, furthermore] that the source of the motive force...is situated in the sun, seeing that it is placed exactly at the centre of the Universe.'

Thus, Kepler's attempt to show a priori that the source of the motion of celestial bodies is identical, as he puts it, with 'the source of light which is the ornament of the whole Universe, as well as the source of heat which invigorates everything', concurred with the above a posteriori results;<sup>2</sup> an agreement which Kepler consistently sought with his unifying methodology.

In accordance with his quest to reveal the true structure of the Universe. and the true motions of celestial bodies, Kepler rejected the Ptolemaic formula, namely, that velocity is proportional to the distance from the equant, and, as we have seen, introduced a formula of his own which reflects the physical causality he was seeking, namely, that the planet's velocity is inversely proportional to the distance from the prime motive body.<sup>3</sup> Kepler proved this new formula - the distance rule - by showing that near the line of apsides the length of the arcs travelled by the planet in equal times varied inversely with the distance of the planet from the sun.<sup>4</sup> He, however, assumed without further examination that the theorem held for any point of the orbit; 'and even later on', as Dreyer points out, 'when he recognized the orbits to be elliptic, he took it for granted that the proof still held'.<sup>5</sup> As it happened, Kepler's theorem is indeed correct, but applicable only for the two extreme points of the apsides where the radius vector is perpendicular to the tangent. To apply the rule throughout

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1. Quoted by Koyré, *ibid.*, p.191.

2. Quoted by Koyré, *ibid.*

3. Koyré, *ibid.*, p.198.

4. Wilson, 1972, pp.99-100; Dreyer, 1953, p.388.

5. Dreyer, *ibid.*

the orbit meant that Kepler failed to realize that the velocity is in fact proportional at any point of the orbit to the perpendicular from the focus to the tangent at the point in question.<sup>1</sup>

Unaware of this mathematical flaw Kepler proceeded to solve the problem of the planet's motion: how far does the planet go along its path in a given time? Since he lacked the mathematics of the calculus, he had to resort to approximation: the times needed for the planet to traverse small equal arcs were approximately proportional to the distances of these arcs from the sun. The calculations were very tedious: he divided the semicircle into 180 arcs of one degree each and computed the distance of each of these arcs from the sun. Using his distance rule, Kepler could make predictions and, in the case of the earth, check them against the results obtained from Tycho's theory. The difference was at most 9 seconds - an encouraging result indeed.<sup>2</sup>

It should however be borne in mind that the number of points on the path is of course infinite and that to each point there corresponds a different distance, and hence different velocity. Thus, a more accurate approximation could be made by calculating the distances, not degree by degree, but minute by minute, or even second by second. This is however a task for the computer and not for the human calculator; hence Kepler's attempt to arrive at a satisfactory result which would bypass this horrendous task.<sup>3</sup> He noted that all the relevant distances are contained within the plane of the orbit and thus to simplify the calculation he replaced the sum to infinity of the distances in question with the surface, or rather the sector of surface, in which they are contained.<sup>4</sup> In other words, the

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1. Ibid.

2. Wilson, 1972, p.100.

3. Koyré, 1973, pp.229-30, 232-33; Koestler, 1961, pp.139-40.

4. Dreyer, 1953, p.388; Koyré, 1973, p.230; Wilson, 1972, p.100.

area of the sector determined by the arc gives, according to Kepler, a measure of all the distances contained within it. Kepler assumed therefore that the areas enclosed within the equal arcs are proportional to the times the planet takes to traverse them. This is the origin of the area rule which has become known as Kepler's second law. (Historically, it preceded the first law.) The area rule is however incompatible mathematically with the distance rule since, as Dreyer points out, 'a sum of an infinite number of lines side by side does not make an area, a fact', Dreyer further notes, 'of which Kepler was quite aware'.<sup>1</sup> To put it differently, the area rule is a completely new principle which emerged from the attempt to circumvent the tedious application of the distance rule. The results of the application of the new principle to the earth's motion differed by 34 seconds from Tycho's theory, a clear vindication of Kepler's new method of approximation.<sup>2</sup>

The case of Mars was, however, yet to be examined. It is to this old problem that Kepler now returns - in the fourth phase of his war - four years after his first, frustrated attempts, with more scepticism with regard to orthodox dogma and with a greater mathematical skill. It is important here to record Kepler's aware<sup>e</sup>ness of the limitations of his method of calculation; and although the following quotation is tinged with hindsight, it does convey the self-critical flavour of Kepler's outlook.

'This method of calculating the equation [i.e., the area rule], Kepler writes, 'is by far the most rapid, and it is based on the causes of natural motions,...furthermore, in the theory of the Sun, or of the Earth, it agrees perfectly with observation; nevertheless, it is false in two respects: primo, it supposes that the planetary orbit is a perfect circle, which is not true, as will be shown later in chapter XLIV (however, if we suppose the orbit to be elliptical, this method has no defects...); secundo, it makes use of a surface which does not exactly measure the distances from the Sun to all points [of the path]. Now, these two causes of error, as though by a miracle, cancel each other [out] in the most exact manner, as will be shown in chapter XLIX.'<sup>3</sup>

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1. Dreyer, *ibid.*; cf., Koyré, 1973, pp.232-33.

2. Wilson, 1972, p.100.

3. Quoted by Koyré, 1973, p.236; for another translation see Aiton, 1969, p.80. Koyré's rendering has a typographical error: Ch.XLIX should be LIX; cf., Dreyer, *op.cit.*



However, the assertion that the errors cancel each other out should not be taken at its face value as indeed some scholars have done. For instance, Dreyer claims that 'the flaw in Kepler's reasoning is curiously <sup>u</sup>counteracted by another one in deducing his law'.<sup>1</sup> Such an interpretation misses the logical structure of Kepler's procedure. As Aiton, following Caspar, points out, 'no question of proof arose at this stage, for at this time Kepler believed the area law to be only an approximation'.<sup>2</sup> In other words, this law, or rather this rule, was at this phase only a mathematical expedience; what Kepler apparently meant was that in the case of the ellipse the area rule would be shown to be exact - that is, a law. Up to this phase Kepler had demonstrated that the equant principle would not do for Mars on the assumption of a circular orbit. But in the case of the earth this principle, as well as the distance and the area rules, led to results which cannot be distinguished by observations.<sup>3</sup> The attack on Mars had to be resumed.

It will suffice for our purposes to give an outline of the remaining four phases of the war on Mars with an emphasis on the sixth phase in which the elliptical orbit emerges as the right orbit. So far we have seen how Kepler's belief in unity and physical realism - a belief which simultaneously demands the search for physical causes and the elimination of abstract constructs as explanatory modes of physical phenomena - made him aware of occurrences of error. That in turn required him to fashion counter-measures; he used independent methods to arrive at the same result, that is, he employed independent tests, and, with a view to determining the quality of the observations involved, he exercised his critical judgement

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1. Dreyer, *ibid.*

2. Aiton, 1969, p.80 note no.31; Wilson, 1968, p.15 note no.51.

3. Wilson, 1968, pp.15-16.

as to what the observations actually consist of: theoretically and practically. As we shall see, the sixth phase of the war brings forth a new element: the emergence of a discovery from an analysis of errors.

Focusing his attention again upon Mars, Kepler applied, in the fourth phase of the war, the distance and the area rules in an attempt to determine the orbital elements of Mars. He assumed a circular orbit and - adapting Ptolemy's epicyclic theory - let Mars move in a circle eccentric to the sun. Kepler expressed scepticism with regard to this arrangement; he did not believe in the classical crystalline spheres and it was thus a mystery for him how the planet could support itself in such an intricate orbit with, moreover, a non-uniform motion. Notwithstanding these difficulties, <sup>Kepler</sup> proceeded to calculate the equations of the orbit by applying the area rule. The results of the calculations showed, in comparison with observations, discrepancies of 8 minutes in the octants. Kepler, it is important to note, checked that the resultant error in the application of the area rule could not account for these discrepancies; for apart from the fact that the magnitude of the difference was too large to come from this source, the error was in the opposite sense.<sup>1</sup> The discrepancies, Kepler concluded, originated in the erroneous assumption that the orbit of Mars was a circle. In other words, Mars was found to move too rapidly at the apsides and too slowly at the quadrants. If the assumption of the circle were to be wrong, then the orbit had to be brought within the circle at the quadrants so that the area would be reduced there and hence the time for traversing a given arc in this vicinity shortened. The orbit would then be an oval.<sup>2</sup>

In the fifth phase of the war, Kepler introduced, for simplification, a mathematical approximation - what he called 'auxiliary ellipse' - in

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1. Aiton, 1969, p.81.

2. Wilson, 1968, pp.15-17 and, 1972, pp.100-101; Dreyer, 1953, pp.389-90; Koyré, 1973, pp.225-240.

place of the mathematically complicated egg-shaped oval. Proceeding with the calculations of the equations of the orbit he found again agreement with observations in the apsides and discrepancies in the octants. In this case, however, the errors were almost the exact opposite of the errors found with the assumption of a circular orbit; that is, the planet was being made to move too slowly at the apsides and too rapidly at the quadrants. Thus, there needed to be more area - area being proportional to time - between the sun and the planet's position on the auxiliary ellipse at the quadrants in order to slow the planet there.<sup>1</sup>

In the sixth phase of the war Kepler arrived at the conclusion that he would get a correct theory for Mars if he had chosen an orbit just midway between the circle of the fourth phase and the auxiliary ellipse of the fifth phase: the resulting elliptical orbit reduces the symmetrical errors in the octants to zero.<sup>2</sup>

As Wilson suggests, Kepler 'idealized' the data at this juncture of his argument: he assumed that the errors in the determination of the anomalies of the orbit were for the circle and the auxiliary ellipse numerically equal but opposite in sign. However, his many different calculations of the anomalies at the quadrants and octants had convinced Kepler that the numerical results were not trustworthy to seconds. Rounding them off to minutes, it emerged that the errors were numerically equal but opposite in sign. The symmetrical errors thus led Kepler to the conclusion that the correct orbit lies precisely in the middle, that is, between the circle and the auxiliary ellipse.<sup>3</sup> This is the only occasion in Kepler's study

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1. Wilson, 1968, pp.17-18 and, 1972, pp.101-102; Dreyer, *ibid.*, pp.390-391; Koyré, *ibid.*, pp.241-264.

2. Wilson, 1968, p.18 and, 1972, pp.102-3; Dreyer, *ibid.*, p.391; Koyré, *ibid.*

3. Wilson, 1968, p.18.

of Mars where a step was taken entirely on the basis of empirical data.<sup>1</sup> As Wilson suggests, it is easy to imagine that Kepler might have chosen the ellipse as the simplest of ovals, had he not been seeking an account in terms of physical causes.<sup>2</sup> Indeed, it is quite probable that had Galileo been presented with this problem he would have decided for the ellipse solely on the basis of the argument of simplicity. He would have arrived at this conclusion, it seems, only after long and hard thinking since it would have been very difficult for him to depart from the simplicity of the circle.

However, for Kepler the argument of simplicity was not of paramount importance; rather, the analysis of the errors in the equations of the orbit, on the assumption of the area rule for the circle and for the auxiliary ellipse of the oval theory, suggested not only the ellipse but a way to determine its dimension. Briefly, as Kepler writes, 'the circle is too wide; the [auxiliary] ellipse is too narrow. The deviation in each case is of the same order; but only another ellipse can be placed mid-way between the circle and the ellipse. Consequently,' Kepler concludes, 'the orbit of the planet is an ellipse, and the [maximum] width of the lune between it and the circle is one-half that of the former case.'<sup>3</sup> Thus, Newton's view that 'Kepler knew ye Orb to be not circular but oval, & guest it to be Elliptical',<sup>4</sup> obscures the innovative feature of Kepler's path to a great astronomical discovery. To be sure, in the absence of a large number of position observations which could be treated

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1. Aiton, 1976, p.88.

2. Wilson, 1968, p.21.

3. Quoted by Koyré, 1973, p.264. Kepler's words are: 'Argumentatio mea talis fuit, qualis cap. XLIX. L. et LVI. Circulus cap. XLIII peccat excessu, ellipsis capitis XLV peccat defectu. Et sunt excessus ille et hic defectus aequales. Inter circulum vero et ellipsin nihil mediat nisi ellipsis alia. Ergo ellipsis est Planetæ iter; et lunula a semicirculo resecta habet dimidiam prioris latitudinem scilicet 429.' (Kepler, 1938-1975, III p.366.)

4. Quoted by Wilson, 1968, pp.5 note nq2, 25.

statistically, the shape of the orbit could not be determined with certainty. However, Newton's view suggests, as Wilson remarks, 'little awareness of the empirical and rational grounds on which that guess was based'.<sup>1</sup> In sum, underlying Kepler's guess there is the perception of a consistent and meaningful pattern of error which the application of the area rule to both the circle and the auxiliary ellipse yielded.

In accordance with his methodology, Kepler checked the agreement between observations and calculations on the assumptions of the area rule and the intermediate ellipse; the agreement was, as Kepler puts it, 'ad unguem'.<sup>2</sup> He used, furthermore, the distance determination to check yet again the intermediate ellipse. In 28 comparisons - positions are predicted by the distance and then compared with observations - the average discrepancy was less than 3 minutes, its maximum being less than 6 minutes.<sup>3</sup> Although Kepler's discovery had emerged unscathed from these tests, Kepler felt that his 'triumph over Mars' was empty. He could not explain why the planet moves in this particular orbit; to complete his victory over Mars, Kepler had to find the physical principles which would govern his discoveries. Thus, it is to 'why' questions that Kepler turns in the seventh and final phase of the war on Mars.<sup>4</sup>

In the light of his new discoveries, Kepler re-examined the distance rule. By distinguishing between what he called 'diametral distance' and 'circumferential distance', he convinced himself of the validity of the distance rule as an expression of a physical principle also in the case

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1. Ibid., p.25.

2. That is to say, on the nail. Quoted by Wilson, 1968, p.21 note no.85; Kepler, 1938-1975, XV, p.250.

3. Wilson, 1972, p.103.

4. Wilson, 1968, p.19.

of the intermediate ellipse.<sup>1</sup>

Kepler concludes the war by claiming that the orbit can only be a perfect ellipse, since here the consequences derived from physical principles are in agreement with observations and the results of the vicarious hypothesis. Elsewhere, in a letter to Fabricius, Kepler reaffirms that the orbit is a perfect ellipse, or at least - he however concedes - it differs insensibly from such an ellipse.<sup>2</sup>

According to Gingerich, Kepler executed a transition 'from model to reality';<sup>3</sup> indeed, the introduction of the elements of linear distance and time into the Copernican system so that the concept of orbit would carry physical meaning attests to this transition. Up to the time of Kepler, astronomy had found interest merely in angular distances between the celestial 'phenomena' - an orbit was only a geometrical construction. For Kepler, however, an orbit bears a physical meaning; 'properly speaking', Kepler writes, an orbit 'is that line which the planet describes around the sun by means of the centre of its body'.<sup>4</sup>

'When you speak of the components of motions', Kepler explains to Fabricius, 'you speak of something which is only imagination, and which does not exist in reality; for nothing performs the circuits in the sky except the body of the planet itself; [there is] no sphere, no epicycle; you,' Kepler reproaches Fabricius, 'who have been initiated into Tychonian astronomy should know that. Now, if we adhere to the fundamental claim that nothing moves except the body of the planet, the question then arises: what is the path traced out by the circumvolution of this body?'<sup>5</sup>

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1. The rule of the diametral distance ('distancia diametralis') - the perpendicular projection of the circumferential distance (the sun-Mars distance of the circular orbit of the fourth phase) onto the corresponding diameter of the circle - shifts the radii vectores in such a way that the distance formula can denote distances in the ellipse. In practice, this reorientation does not contradict the observationally determined distances as the shift never exceeds 8 minutes of arc. (Wilson, 1972, pp.103-6 and, 1968, pp.19-21; Dreyer, 1953, pp.391-93; Koyré, 1973, pp.253-64.)
  2. Wilson, 1968, p.21, notes no. 79 and 80; Koyré, *ibid.*, p.261.
  3. Gingerich, 1973a, p.520.
  4. Kepler, 1952, p.999.
  5. Quoted by Koyré, 1973, p.263.

Clearly, for Kepler an orbit is not merely an abstract construction, but primarily a physical phenomenon whose elements have to be determined a posteriori from a priori reasons, that is, as stemming from physical causes.<sup>1</sup>

In response to the criticism which Maestlin - Kepler's teacher - had directed towards Kepler's concept of physical causes, Kepler reasserted that

'contrary to what Ptolemy assumes, neither an eccentric nor an epicycle would exist in the heavens, and the Earth would not be moving as Copernicus assumes. Furthermore,' Kepler continues his reply, 'with such false assumptions as a basis, it would be a wonder if astronomers could truly predict the positions of the planets. On the other hand, I claim,' Kepler writes, 'that all astronomers speak a certain amount of truth - one more than another - and in order to proceed more correctly with the calculation many things are indeed accepted that Nature does not know about in the sky. However,' Kepler stipulates, 'I shall accept only that which cannot be doubted as truly real, and therefore physical, keeping in mind the nature not of the elements, but of the heavens. If I wholly reject the perfect eccentrics and epicycles, I do so because they are purely geometrical assumptions that do not correspond to any body in the sky.'<sup>2</sup>

Probably unaware of Bacon's warning against the peculiar disposition of the human intellect, Kepler took cognizance precisely of this: 'The simplicity of Nature,' he instructed Fabricius, 'must not be judged by our imagination.'<sup>3</sup> It appears that Kepler did indeed give heed to the pitfalls classified in Bacon's categories of the idols.

A proverb has it that liars are cautioned to remember what they have said, lest they betray themselves; likewise, Kepler believes, false hypotheses which led to a correct conclusion would end up by betraying themselves when applied to ever more varied cases.<sup>4</sup> Kepler claims that it is in the domain of physics, and not within the bounds of geometry or astronomy alone, that an hypothesis can be put to the most vigorous test. (Recall the fate

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1. Heisenberg, 1958, p.78.

2. Quoted by Danilov and Smorodinskii, 1975, p.703.

3. Quoted by Koyré, 1973, p.263.

4. Kepler, 1858-1871, I p.239. See Duhem, 1969, p.102; Jardine, 1984, p.140.

of the vicarious hypothesis.) Kepler reproaches therefore his contemporaries who, while confining their thinking within the limits of geometry or astronomy, discussed futilely the question of equivalence of hypotheses. It was futile since, as Kepler maintains,

even if the conclusions of two hypotheses coincide in the geometrical realm, each hypothesis will have its own peculiar corollary in the physical realm. But practitioners are not always in the habit of taking account of that diversity in physical matters.<sup>1</sup>

While unifying geometry and physics in his astronomical studies, Kepler undoubtedly shifted the burden of proof to physics.<sup>2</sup>

In the light of this discussion we may conclude that Kepler's epistemology and methodology - which I view as an attempt to reconcile, in the pursuit of unity, the doctrines of Plato and Aristotle - facilitated the understanding of the shortcomings of one's knowledge when one endeavours to amalgamate the abstract with the concrete; that is, as Kepler I believe perceived it, the amalgamation of mathematics and physics. Against such a philosophical background the problem of experimental error is bound to come to the fore. As Einstein - a philosopher-scientist whose philosophical make-up resembles that of Kepler - succinctly puts it: 'as far as the propositions of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality.'<sup>3</sup> In Kepler's new astronomy - an astronomy based on physica coelestis - mathematical terms do refer to the physically real; thus, errors, and in particular experimental errors - be they theoretical or practical - are recognized

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1. Jardine, *ibid.*, pp.141-42. Kepler's words are: 'Nam si in Geometricis duarum hypothesium conclusiones coincidant, in physicis tamen quaelibet habebit suam peculiarem appendicem. Sed quia ab artificibus illa in physicis varietas non semper considerari solet.' (Jardine, *ibid.*, p.90.)
  2. Westman, 1972, p.239.
  3. Einstein, 1981, p.228.



as part and parcel of one's inquiry into nature.<sup>1</sup>

By all accounts, Kepler's Astronomia Nova is a theoretical work. In that study Kepler attempted to cast the observational results of Tycho Brahe into a theoretical framework which, he hoped, would reveal the underlying laws of planetary motions. Indeed, the prime objective of the Astronomia Nova is, briefly, the successful matching of calculations with observations. In this context, errors amount to discrepancies and do not bear the character of, for instance, instrumental failure or erroneous transit reading. To be sure, discrepancies may originate in such failures, but in a theoretical work like that attention is focused on the gap between the result of the calculation and the relevant observation. Thus, although Kepler himself took a few observations, he was much concerned, in the Astronomia Nova, with calculations, analyses, interpretations and indeed theoretical background: for the failure of the vicarious hypothesis had taught him the lesson that one has to be aware of theoretical assumptions which one takes for granted.<sup>2</sup> Consequently, the errors with which Kepler grappled in the Astronomia Nova do not usually pertain to the observational instruments and the actual process of observation; in other words, they are mostly theoretical.

An impression may thus be formed that Kepler was not aware of experimental errors other than the theoretical ones; such an impression is however totally misguided. The following discussion is intended therefore to counter

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1. Characteristically, Ptolemy detaches mathematics from the physically real and stresses the superiority of mathematics over physics. In the introduction to the Almagest, Ptolemy writes: 'While the two types of theory could better be called conjecture than certain knowledge - theology because of the total invisibility and remoteness of its object, physics because of the instability and uncertainty of matter ...mathematics alone... will offer reliable and certain knowledge because the proof follows the indisputable ways of arithmetic and geometry.' (Quoted by Neugebauer, 1983, p.37.) Ptolemy's astronomy is indeed mathematical; it is not based on any physical concept similar to the physica coelestis of Kepler.

2. Supra, Ch.III, pp.103ff.

this false view, and indeed to show that Kepler's awareness of the problem of experimental error calls for a wider classification than the one Lloyd applies to Greek astronomy.<sup>1</sup>

Kepler himself is quite explicit about the gap between theory and practice. Responding in 1610 to Galileo's employment of the telescope, Kepler writes that he is

aware how great a difference there is between theoretical speculation and visual experience; between Ptolemy's discussion of the antipodes and Columbus' discovery of the New World, and likewise between the widely distributed tubes with two lenses and the apparatus with which ... Galileo [has] pierced the heavens.<sup>2</sup>

Eight months after this response, Kepler - prescribing a hyperbolic lens to eliminate spherical aberration - informs a correspondent that this theoretical advantage might not be attained in practice:

All our artisans agree in asserting that a hyperbolic lens cannot be turned on the lathe. For, the grooves become circular as the hyperbolically shaped scraper's innermost projection, being the axis of the rotating conoid, rubs practically nothing away, whereas the outermost parts scrape away very much material. Nor can this situation be remedied by varying the contacts. For, a portion of a sphere can be applied everywhere to the hollow part of the blank which is being ground down by the same spherical surface, both at its center or axis and at its outermost edge. This is not the case in the usual manufacture of conoidal shapes. For, only the projecting tip presses into the center or bottom of the hollow blank. . . But it will not be difficult for Galileo to devise a new kind of machine by which also the bottom of the concave blank may be dug out more vigorously than it is usually done by the almost stationary parts around the axis of the motion. Such a process I too have at hand already, and if I succeed with what I have started, perhaps I shall undertake to make it with my own hands.<sup>3</sup>

Evidently, Kepler did not pursue theoretical considerations per se; he was aware of their practical implications and indeed acknowledged the difficulties in executing them. Before outlining such difficulties in optics, it is however instructive to examine Kepler's view of optics.

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1. Supra, Ch.II, pp. 77-79.

2. Quoted by Rosen, 1965, p.17.

3. Quoted by Rosen, *ibid.*, p.89 note no.157.

It may be recalled that in the initial stage of his war on Mars, Kepler set himself the task of determining the parameters of the orbit of Mars out of four sets of opposition observations. Attempting to solve this geometrical problem iteratively, Kepler implored his reader to take pity on him since 70 trials had been needed with the loss of much time; indeed, he reports that 'this already is the fifth year since I have attacked Mars, although the year 1603 was almost entirely spent on optical investigations'.<sup>1</sup>

Amidst his determined attack on Mars, Kepler paused to inquire into a science whose relevance to astronomy cannot be ignored, namely, optics. And, indeed, in 1604, Kepler published this inquiry under the seemingly humble title: 'Additional Remarks on Witelo in which is Discussed the Optical Part of Astronomy'; chiefly concerning the artificial observation of the diameters and eclipses of the sun and moon. With examples of famous eclipses.' In the form of an advertisement, the title-page bears, furthermore, a personal remark to the reader that he will find in this book, 'inter alia, many new things, an illuminating treatise on the mode of vision and on the function of the liquids of the eye, contradicting eye specialists and anatomists.'<sup>2</sup> The so-called Additional Remarks (Paralipomena) constitute in fact a new optics which would have secured a place for Kepler in the annals of science even if he had not published any other work.

In his astronomical studies, Kepler realized that the law of refraction is crucial for correcting errors in astronomical observations arising from atmospheric refraction. With respect to the search for this law, the Paralipomena echo the methodological struggle through which Kepler went in his attempt to unravel the secrets of Mars. Like the Astronomia Nova, the Paralipomena follow the historical development of Kepler's thoughts. After detailing all the blind alleys he had pursued: curve-fitting, trial

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1. Supra, Ch.III, p.102 note 4.

2. See the title-page in Ronchi, 1970, Fig.16.

and error, and analogical reasoning, Kepler returned, as in the Astronomia Nova, to causal physical mechanism.<sup>1</sup> He sought an internal relationship between the laws of refraction and reflection, and attempted to analyse refraction by means of conic sections.<sup>2</sup> It is in this context that the term 'focus' appears for the first time.<sup>3</sup>

However, as Whiteside points out, the central problem in the Astronomia Nova of sieving and reducing an abundance of observed measures of a class of phenomena - before setting some mathematical or analogical structure of explanation upon it - has no counterpart in the sections on refractive optics in the Paralipomena. The so-called empirical data which consist of only eight pairs of angles of incidence and refraction at an air/water interface, are borrowed from a tabulation in Witelo's Perspectiva; the latter being no great improvement upon Ptolemy's results. Had Kepler 'been heir to the Nachlass of some optical Tycho who had amassed accurately observed values of refraction at a variety of interfaces, he,' Whiteside suggests, 'might well have emulated Harriot by inducing some equivalent to the general sine-rule.'<sup>4</sup>

Another case which can shed light on Kepler's view of optics is the occurrence of annular eclipse. In 1567, the astronomer Christopher Clavius (1538-1612) reported that the total solar eclipse which he had observed in that year in Rome, was annular: the moon was surrounded by a ring of sunlight. The implication of this observation was that Ptolemy's claim which had prevailed throughout the years that the apparent diameter of the moon cannot be smaller than that of the sun, was in fact in error.<sup>5</sup> If Ptolemy's claim were correct then one would expect that in a total solar eclipse the disc of the moon

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1. Buchdahl, 1972, p.287.

2. Lohne, 1975, p.858.

3. Wilson, 1968, pp.13-14.

4. Whiteside, 1974, p.390.

5. Supra, Ch.II, p.75 note no.3.

should have always obscured the sun; an expectation which did not materialize in Clavius' observation. Consequently, new measurements were called for.<sup>1</sup>

Kepler responded to this call by analysing, theoretically as well as experimentally, the instrument with which such measurements had been taken, namely, the camera obscura. Moreover, he went on to invent the instrumentum eclipticum which he designed to facilitate the attainment of greater accuracy in the observations of eclipses in general and the measurement of the angular diameters of the sun and the moon in particular.<sup>2</sup>

Another important contribution of Kepler to optics is the theoretical underpinning which he developed for the Galilean telescope, and the substantial improvements which he made upon its design so that greater accuracy and more reliable results would be obtained.

Underlying the search for the law of refraction, the optical analysis of the camera obscura and the theoretical underpinning of the telescope, lies Kepler's general view of light as rays which are propagated in all directions from all points of a source of light and, in diffuse reflection, are further propagated, again in all directions, from each point of the reflecting surface.<sup>3</sup> As Buchdahl remarks, this view requires a distinction between the light ray as a purely geometrical notion - that is, nothing but the motion of light - and light proper, which exists in the form of a spherical surface and has some kind of physical reality.<sup>4</sup> The emphasis upon the motion of light rather than on light itself, allowed Kepler to render the eye an optical instrument and to divorce the formation of the image on the retina from its mental perception. By doing so Kepler was able to confer objectivity upon astronomical observations, notwithstanding his awareness of the differences in individual eyesights.<sup>5</sup>

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1. Marek, 1973, p.216; Marek, 1975, p.850.

2. Infra, pp.129-131.

3. Aiton, 1976, p.92.

4. Buchdahl, 1972, p.271.

5. Infra, pp.134-135.

These cases show clearly that Kepler's inquiry into the field of optics was directly influenced by his astronomical preoccupation. His lack of concern for the phenomenon of colour – perhaps the most striking phenomenon in this field of study – confirms the claim that Kepler's optical studies were carried out with applications to astronomy in mind.<sup>1</sup> However valuable his contribution to optics per se, his researches in this field were dictated by his astronomical interests. Kepler, it appears, was concerned with the properties of light in so far as the understanding of them rendered service to astronomy in securing more accurate and reliable observations. Kepler thus perceived optics as an auxiliary science which was designed to assist in his astronomical researches.<sup>2</sup>

Over and above Kepler's general view of optics, we find examples which demonstrate his awareness of the ramification of the problem of experimental error. It was Maestlin who, at the University of Tübingen, acquainted Kepler with the use of the camera obscura for astronomical observations. He used the loft of a church as the camera and a hole in the wall as the aperture. A sheet of paper, held by Maestlin's assistant, functioned as the screen.<sup>3</sup> Kepler's invention of the instrumentum eclipticum seems to have originated in his attempt to improve Maestlin's method and make the device more suitable for his own observations. The device consisted of a rod in the role of an axis, on the ends of which were fastened perpendicularly a plate provided with an aperture and a screen; the distance between them being adjustable.<sup>4</sup>

In the Paralipomena Kepler refers to Clavius' description of the 1567 eclipse and explicitly states his belief in the correctness of the result

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1. Linnik, 1975, p.812.

2. Marek, 1973, p.215; Marek, 1975, p.852.

3. Ibid., 1973, p.217.

4. Linnik, op.cit., pp.813-14.

of this observation.<sup>1</sup> Using his own device, Kepler observed in 1600 the eclipse of the sun and noticed, in spite of his weak eyesight, the effects of diffraction: the observation of the sun's crescent with the naked eye differed from the observation of the image of the crescent projected in the camera obscura; while in the former case the horns of the crescent were sharp, in the latter they were rounded. Furthermore, the eclipse was continuous when observed with the naked eye, whereas its projected image in the camera obscura was discontinuous: the phase of full eclipse being reached abruptly from a partial eclipse.<sup>2</sup> Kepler carried out several experiments to clarify these observations, but it was left to Grimaldi (1618-1663) who followed up Kepler's researches in optics, to discover the phenomenon of diffraction.<sup>3</sup>

What intrigued Kepler most was the fact that different magnitudes of the diameter of the projected image of the moon had been obtained. He rejected the explanation that the cause of these differences was due to the variable distance of the moon, and at first looked for errors and mistakes in his measurements and calculations.<sup>4</sup>

He quickly realized that the magnitude of the projected image is dependent on the geometrical arrangement of the observation; that is, the parameters of the camera obscura affect the magnitude of the projected image. He therefore proceeded to study the way in which the image depends on geometrical factors, and thus examined factors such as the size and shape of the aperture, and the distance between the aperture and the screen.<sup>5</sup>

In this study Kepler demonstrates that the aperture itself is projected onto the screen, and that this additional projection interferes with the projected image of the celestial body in such a way that the magnitude

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1. Marek, 1973, p.217.

2. Ibid.

3. Marek, 1975, pp.852-53.

4. Marek, 1973, p.217.

5. Ibid., p.218.

of the latter is enlarged. Thus, the correct magnitude of the apparent diameter of a celestial body projected in the camera obscura should be obtained by subtracting the magnitude of the diameter of the aperture used, from the measured magnitude of the diameter of the projected image. Kepler thought this result to be sufficiently important to warrant quick publication, and he rushed the Paralipomena through the press so that it would be available to astronomers in time for the eclipse of 1605.<sup>1</sup>

However, the fifth chapter of this book - entitled 'De Modo Vision' - contains perhaps an even greater landmark in the annals of optics. It opens with an accurate description of the anatomy of the eye, and states that 'vision takes place when the image of the whole hemisphere of the world in front of the eye..., is formed upon the concave reddish surface of the retina'.<sup>2</sup> According to the historian of optics V. Ronchi, for the first time in the history of this subject there was no hesitation in letting the light stimulus arrive directly at the retina.<sup>3</sup> Kepler's predecessors had followed Alhazen's view that the surface of the crystalline lens facing towards the pupil is the sensitive surface; they thus avoided the notion of inverted image on the back of the eye - a notion which they considered disturbing. Kepler, taking perhaps his cue from the analysis of the camera obscura, had 'renounced and abjured' this prejudice - as Bacon later demanded in his Novum Organum to do with any prejudice<sup>4</sup> - and thereby revolutionized optics.<sup>5</sup>

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1. Ibid., pp.218-19; Marek, 1975, p.852; Straker, 1981. In his Somnium, Kepler gives instructions for observing solar eclipses. 'Observers should... be warned that the paper which receives the image of the eclipsed sun must be protected from all disturbances and must always be placed at the same distance from the hole and at right angles to the ray coming through it. For if the paper bends, the circumferences of the bright image are distorted, and degenerate from circles into ellipses. Accordingly let the disputant verify whether he took adequate precaution against this defect.' (Quoted by Sheynin, 1973, p.106.)
  2. Quoted by Ronchi, 1970, p.91.
  3. Ibid.
  4. Supra, Ch.I, p.18 note no.7.
  5. Ronchi, op.cit., pp.91-92.



There are two points worth stressing for our purposes with reference to Kepler's study of the process of seeing. The first point is his attempt to transcend the subjective element of perception, and the second one is his dealing with this very aspect of observation, namely, its subjectivity.

Employing his general view of light, Kepler studied the cones of rays which have their apexes at the various points of an object and their bases, common to all, in the pupil. He called a section of such a cone triangulum distantiae mensorium, since in his view such a triangle supplied the observer with all that was required for locating the observed point at its vertex. He followed the refraction of these rays through the cornea and the crystalline lens, and showed that each of these cones changed by refraction into another cone whose base and apex were at the pupil and on the retina respectively. Thus, the direction of the rays arriving at the cornea from the point-object is linked to the position of the retinal point which receives the stimulation. For if that direction changes, the stimulated point on the retina changes too. This analysis, which is essentially a triangulation process, established firmly the correspondence between the points of the object and those of the image on the retina.<sup>1</sup>

However, while experimenting with transparent spheres - which interested him as corresponding to the eyeball - Kepler realized that his theory could hold only if a distinction were to be made between images whose rays can be intercepted on a screen and those which the eye receives directly. The former he called 'pictures', while those seen directly by the eye when it looks at mirrors, prisms and lenses, he termed 'images of things'.<sup>2</sup>

As Kepler found out, those images received directly by the eye did not submit to a simple uniform generalization; he thus admonished his fellow

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1. Ronchi, 1957, pp.43-44; Ronchi, 1970, pp.90-92.

2. Ronchi, 1957, p.45; Ronchi, 1970, p.93.

scientists, as Ronchi observes, that they should forget about the 'images of things' and deal only with the 'pictures'.<sup>1</sup>

Experimenters who used to observe directly in mirrors, prisms and lenses, could not reach conclusive results. The phenomena they studied were too diverse and dependent on many conditions. As Ronchi explains, 'the psychological intervention of the observer predominated to such an extent that it was impossible to arrive at any decisive factor of a physical nature'.<sup>2</sup> Since the subjective element in locating the 'images of things' was too dominant, the conditions were too arbitrary to provide a basis for enunciating a physical law.<sup>3</sup> However, by concentrating on the 'pictures', Kepler could solve optical problems without involving the observer's eye, let alone his mental state.<sup>4</sup> Kepler's approach permitted therefore the construction of an objective optics in the sense that it was independent of an observer. To be sure, the act of seeing was done by an observer, but his task was fixed: to locate the luminous point at the vertex of the cone of rays reaching the cornea. This fixed task was not affected by physio-psychological aspects and their exclusion was thus permitted.<sup>5</sup>

Being critical of this approach - an approach which in a sense eliminates the human eye - Ronchi exclaims that such an optics is also valid for blind men.<sup>6</sup> Kepler, however, left the study of perception to other disciplines. He was quite aware of the problem of transmission: the process whereby the retinal image is transmitted to the brain; but he stated clearly that

how this portrait or this picture links up with the visual spirits dwelling in the retina and the nerve; whether it is by these spirits that it is led through the hollow passages of the brain to appear before the tribunal of the soul or of the visual faculty, or whether on the contrary it is the visual faculty or deputy appointed by the

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1. Ronchi, 1957, p.204.

2. Ibid.

3. Ibid., p.276.

4. Ibid., pp.205, 276.

5. Ibid., p.50.

6. Ronchi, 1963, p.622.

soul which descends from the judgement hall of the brain into the optic nerve itself and into the retina, or even to the lowest benches of the tribunal, going out to meet this image; certainly I leave that to the physicians to decide.

However, Kepler's attempt to confer objectivity upon observation, and thus to eliminate a persistent confusion arising from the subjectivity of the observer, did not prevent him from acknowledging differences in individual eyesights. Indeed, he was quite explicit about his own defective and weak eyesight. As he explained, he suffered from multiple vision; 'instead of a single small object at a great distance, two or three are seen by those who suffer from this defect. Hence instead of a single moon', Kepler reported, 'ten or more present themselves to me'.<sup>2</sup> Informing a correspondent of his defect, Kepler explained further that it occurred only in strong light. 'When the moon is bright,' Kepler wrote, 'it spreads itself out in my vision by being multiplied into many circles intertwined with one another so as to look like a bigger sphere; but where the moon is dim, there that multiplication stops.'<sup>3</sup> Kepler reported further that he was near-sighted and used concave lenses of moderate curvature to improve his eyesight.<sup>4</sup>

Kepler took into consideration these differences in eyesights when he designed his extensible telescope. He explained that there was a labour-saving alternative to changing the concave lenses in his telescope for every observer.

'Since eyes differ in strength,' he wrote, 'a variable distance between the lens could help everybody. For, one man pulls the tube out further than another, in order to see clearly.' And he explained that 'if the concave lens is separated from the convex lens by the same distance and is so fixed that it cannot be moved out of its position, it does

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1. Quoted by Simon, 1975, p.827.
  2. Rosen, 1965, pp.71-2 note no.88.
  3. Ibid; cf., pp.35, 130 note no.302.
  4. Ibid., p.72 note no.88.

not suit everybody's eyes, but with such an unadjustable tube, the concave lens will have to be changed. However, a most useful short cut is provided by the opportunity to vary this distance by means of a tube that can be pulled out and pushed together. For in this way the same concave lens in a variable position takes the place of many concave lenses in one and the same position.'

Being aware of the defects of eyesight, Kepler considered how they might affect the quality of astronomical observations; indeed, he spoke of 'the true motions of the planets unadulterated by the distortion of the sense of sight'.<sup>2</sup> He further examined, as Linnik reports,

the effect of the position of the eye upon the accuracy of angular measurements between star pairs; the apparent enlargement of the angular size of a bright object [irradiation]; and the disappearance of a faint object in the neighbourhood of a bright one.<sup>3</sup>

But perhaps most significant for our theme of error is the method he employed to check Galileo's telescope. Kepler made his observations in the company of friends and helpers, and instructed each person to observe and record his results separately; having taken the observations separately the individual results were then compared.<sup>4</sup> Although this method is not dissimilar to the measurement of personal equations – conceived by Bessel two centuries later<sup>5</sup> – its prime purpose was rather to establish a certain inter-subjective ground upon which the disputed observational results could be accepted.

In the cases I have mentioned, one can see Kepler attending to optical problems which had arisen in the process of taking astronomical observations. He tackled them first as a theoretician, but then proceeded to determine the practical implications of his theoretical analyses. He thus came up with a correct method for measuring the apparent diameter of a celestial

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1. Ibid., pp.90–1 note no.162; cf., pp.20–21.

2. Jardine, 1984, p.154. Kepler's words are: '... verj planetarum motus, non adulteratj visus commutatione...' (Ibid., p.98.)

3. Linnik, 1975, p.812.

4. Marek, 1975, pp.853–54.

5. Infra, Ch.IV, pp.176ff.

body projected in the camera obscura, and attempted to transcend the subjective element - a source of many errors and confusions - which inheres in any observation. But above all, it is the way he responded to Galileo's pioneering use of the telescope which epitomizes this characteristic of Kepler, namely, the constant alert to both theoretical and practical pitfalls. It is to this case that I now turn before concluding the account of Kepler's insight into the problem of error in science.

The telescopes which the Dutch craftsmen put into circulation early in the seventeenth century were of poor quality: they magnified disproportionately<sup>1</sup> and produced distorted images which were falsely coloured; they did not give therefore any confidence in what they were supposed to do. Like spectacle lenses, the telescopes came up against the combined opposition of mathematicians and philosophers who stressed their gross faults and presented them as unworthy of consideration as a means of scientific research.<sup>2</sup> Indeed, the Paralipomena reflect this attitude quite clearly; they deal only in passing with lenses, considering them useful devices for increasing the convergence or divergence of the cones of rays, and therefore only suitable for correcting defects of vision. Thus, in 1604, even Kepler did not have any particular interest in lenses; and although he was the first scientist to give the correct analysis of the way spectacle lenses restore normal vision, he reported that had it not been for Ludwig of Dietrichstein - a high official in Prague who for three years tried to interest Kepler in the question of spectacle lenses - he would not have undertaken such a study.<sup>3</sup>

Before the discoveries of Galileo were brought to his attention, Kepler had been of the opinion that with Tycho Brahe's observations 'the pinnacle had

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1. The magnification was no more than three times; for comparison, the Galileo telescope attained a magnification of thirty times. (Ronchi, 1963, p.549.)
  2. Ibid., pp.548-49.
  3. Ronchi, 1970, pp.94-95.

been reached, and that nothing further was left to human enterprise, because,' as he explains in his famous Conversation with Galileo's Sidereal Messenger, 'the eyes would not permit greater precision, nor would the effect of refraction, which alters the position of the stars with reference to the horizon.'<sup>1</sup> When, however, it was put to him that one day somebody would devise a more exact procedure with the help of lenses, he objected on the ground that their refractive properties made them unsuitable for reliable observation.<sup>2</sup>

In his Conversation, Kepler disclosed further that he had been highly critical of Della Porta's (1534-1615) suggestion of attempting an extension of vision to unimaginable distances. He had rebuked Della Porta particularly for thinking that lenses might supply or increase the light to make things visible. 'Is it true', Kepler asks rhetorically, 'that no lens can ever detect objects which do not of themselves impart to our eyes some degree of light as the medium through which the objects acquire visibility'.<sup>3</sup> As he had understood it, it was vain to hope that a lens would remove the substance of the intervening dense air which obscures and distorts minute parts of visible things at a distance. For such reasons and the like, he had refrained, as he stated, from attempting to construct the device.<sup>4</sup>

However, when he learnt of Galileo's discoveries with the telescope, he did attempt to build it but was unsuccessful in doing that. In August, 1610, the Elector of Cologne, who was seeking Kepler's views of these discoveries, sent him one of those Italian telescopes which Galileo had used. Being in possession of this revolutionary device, Kepler proceeded to undertake a series of tests. Clearly, his intention was to establish, first and foremost, how reliable and accurate this device was.<sup>5</sup> It was

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1. Rosen, 1965, p.21.

4. Ibid.

2. Ibid.

5. Ronchi, 1963, p.551.

3. Ibid., p.18.

in the process of these tests that he engaged other observers so that the results would attain some kind of inter-subjectivity. Having experimented over a period of ten days, he concluded that Galileo was right.

'Most accomplished Galileo,' Kepler addressed him, 'You deserve my praise for your tireless energy. Putting aside all misgivings, you turned directly to visual experimentation. And indeed by your discoveries you caused the sun of truth to rise, ... and by your achievement you showed what could be done.'<sup>1</sup>

Having established the merits of the Galileo telescope, Kepler conceded that Galileo's observations had surpassed Tycho Brahe's attainments of discerning 'celestial degree in the heavens'.<sup>2</sup> For the telescope, as Kepler acknowledged, had subdivided Tycho Brahe's observations with the utmost nicety into minutes and fractions of minutes.<sup>3</sup> However, in Kepler's view, the observational method of Tycho Brahe was sufficiently good to deserve 'a most appropriate marriage', between a method and an instrument. 'You must base your method on Brahe's,' Kepler instructed Galileo, and hoped that 'from this partnership may there some day arise an absolutely perfect theory of the distances.'<sup>4</sup>

Kepler thus joined forces with Galileo against, as he put it,

'the obstinate critics of innovation, for whom anything unfamiliar is unbelievable, for whom anything outside the traditional boundaries of Aristotelian narrowmindedness is wicked and abominable, [and] you may advance,' he encouraged Galileo, 'reinforced by one partisan.'<sup>5</sup>

Galileo, on his part, acknowledged this expression of faith; 'I thank you,' he wrote to Kepler, 'because you were the first one, and practically the only one... to have complete faith in my assertions.'<sup>6</sup>

Characteristically, and in contrast to Galileo, Kepler found it wanting that there was no instrumental theory to explain the observations with the telescope. He had already tried, in his reply to Galileo, to introduce a few theoretical underpinnings which he had based on his results in the Paralipomena. He had attempted to correct aberration, and clearly stated

1. Rosen, 1965, p.18.

4. Ibid., p.22.

2. Ibid., pp.21-22.

5. Ibid., p.12.

3. Ibid.

6. Ibid., p.71 note no.85.

that his 'intention is to avoid... distortion and confusion of the parts of the object under observation'.<sup>1</sup> As he wrote to Galileo, 'the aim is to achieve undistorted vision, in which the images of the parts of the object under observation are enlarged proportionally'.<sup>2</sup>

To achieve this aim Kepler proceeded to advance a theory of lenses and telescopes. Early in 1611, he published his Dioptrice - a thin volume of eighty pages - in which he set down the fundamental principles of lenses and their systems. These principles, as Mach remarks, 'form the first foundation for the theory of optical instruments'.<sup>3</sup>

Kepler introduced the term 'dioptrice' to indicate that this study was of the optics of refracting media. The structure of the work itself consists of definitions, axioms, problems and propositions. Axioms are for Kepler optical theorems which cannot be proved since they are necessities of thought or established experience; problems correspond to experiments and propositions are theorems which can be deduced by logical arguments from definitions and axioms. The Dioptrice deals with plane crystal, curved planes, the lens, focus, the process of seeing and the application of the lens with respect to the eye: convex lens, two joined convex lenses, concave lens and a combination of convex and concave lenses. The penultimate section formulates the following problem: to design a telescope of which both lenses are convex; the last section deals with the problem of making a telescope with the lens towards the eye convex, and that towards the object concave. These discussions of Kepler stimulated Scheiner to actually construct astronomical and terrestrial telescopes, which he successfully produced in 1613.<sup>4</sup>

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1. Ibid., pp.19-20.

2. Ibid., p.20.

3. Mach, 1926, p.53; see the title-page in Ronchi, 1970, fig.27.

4. Görlich, 1975, p.840; Mach, *ibid.*, pp.53-54.



It is quite clear that Kepler used his theoretical insight to improve upon the Galileo design. Specifically, the Kepler telescope had the advantages of providing a larger field of view, and higher magnification; but above all, it permitted the insertion of a thread, or cross hairs, at the back focal plane of the objective, where the real image - the 'picture' - of the observed object is formed. As Chenakal remarks,

with the thread or the centre of the cross hairs aligned on the optic axis of the tube, the instrument could be set very accurately on an observed object or an individual part of the object; thus in turn a telescope of this type could be used as a viewfinder in astronomical angle-measuring instruments, instead of viewfinders with sights leading to a substantial rise in accuracy.<sup>1</sup>

The Galileo telescope, in contrast, had to be set on a celestial body by looking through a narrow slit, so that the opportunity of using the whole field of view of the telescope to search for an object was lost, and the observer was greatly inconvenienced. It is thus no wonder that after the Kepler telescope had made its appearance, it became an inseparable part of every angle-measuring instrument; and that it eventually superseded the Galileo telescope.<sup>2</sup>

As an astronomer, Kepler considered himself to be a priest of the highest God regarding the Book of Nature;<sup>3</sup> in his youth, as a student of theology, he had certainly read the Book of Scripture; but above all, he read the books of ancient and contemporary writers. By way of conclusion, it is worth mentioning one such writer whom Kepler revered and referred to as divine. Nicholas of Cusa had created, together with Pico della Mirandola, that philosophical framework within which, I suggest, Kepler pursued his studies.<sup>4</sup> In his celebrated De Docta Ignorantia (1440), that

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1. Chenakal, 1975, p.822; cf., Görlich, *ibid.*

2. Chenakal, *ibid.*

3. Baumgardt, 1952, p.44.

4. Kepler referred to the doctrines of the 'divine Cusa' (divinus mihi Cusanus) already in his first work, Mysterium Cosmographicum. See Kepler, 1981, pp.19, 23, 93; Aiton, 1973, p.285. For a different view see Rosen, 1965, p.64 note no.56.

is, Of Learned Ignorance, Cusa - the philosopher of unity who believed in the coincidence of opposites - inaugurated the thesis that a finite being, that is, the human mind, can only approach the truth asymptotically: there is no precise knowledge of any of God's work.<sup>1</sup> According to Cusa,

[It] is not the case that by means of likenesses a finite intellect can precisely attain the truth about things. For truth is not something more or something less but is something indivisible. Whatever is not truth cannot measure truth precisely... Hence, the intellect, which is not truth, never comprehends truth... precisely... For the intellect is to truth as [an inscribed] polygon is to [the inscribing] circle... Therefore, the quiddity of things, which is the truth of beings, is unattainable in its purity; though it is sought by all philosophers, it is found by no one as it is. And the more deeply we are instructed in this ignorance, the closer we approach to truth.<sup>2</sup>

Since absolute equality is a predicate of God alone, argues Cusa, there exists a necessary difference between the measure and the measured.<sup>3</sup> Hence,

'we are always using conjecture,' Cusa maintains, 'and err in the results [of our measurements]. And [if] we are surprised when we do not find the stars in the places where they should be according to the ancients, [it is] because we believe [wrongly] that they were right in their conceptions concerning the centres and poles as well as in their measurements.'<sup>4</sup>

Kepler recognized these limitations and pitfalls. He therefore considered himself an adherent of a sound philosophy who would, as he remarked, most heartily congratulate the astronomer should he discover the

'reasons... for our deluded sight perceiving those regular motions otherwise than they have in reality been ordained,' and thus distinguish 'the true motions of the planets from those which are accidental and derived from the phantasms of the sense of sight.'<sup>5</sup>

Kepler's last remark in the 'Notice to the Reader' of the Conversation, bears witness to his sensitivity to other ideas, his broad-mindedness and his explicit awareness of errors.

1. E.g.: 'the ordering of the heavens... is not precisely knowable'. (Nicholas Cusanus, 1981, p.88 (Bk.II, Ch.1).) Cf., Aiton, *ibid.*, p.286; Bett, 1932, Ch.III; Hopkins, 1978, p.133; McTighe, 1964, pp.619-20.
2. Cusanus, 1981, pp.52-53 (Bk.I, Ch.3).
3. *Ibid.*, p.87 (Bk.II, Ch.1); 1954, p.67.
4. Quoted by Koyré, 1974, p.14; cf., Cusanus, 1981, p.115 (Bk.II, Ch.11).
5. Jardine, 1984, p.155. (Kepler's words are: '... si causas deceptionis invenerint, a veris planetarum motibus accidentarios et ex phantasia visionis ortos sejunxerint,...' (Jardine, *ibid.*, p.99.)) Cf., Cassirer, 1964, pp.139-40.

'Let no one assume,' he instructs the reader, 'that by my readiness to agree with Galileo I propose to deprive others of their right to disagree with him. I have praised him, but all men are free to make up their own minds. What is more, I have undertaken herein to defend some of my own views.... I have done so with a conviction of their truth and with serious purpose. Yet I swear to reject them without reservation, as soon as any better informed person points out an error to me by a sound method.'<sup>1</sup>

It emerges from the present discussion that Kepler dealt with a variety of aspects of the concept of experimental error. While casting the observations of Tycho Brahe into a new physical astronomy, Kepler encountered the problem of discrepancy. He neither disregarded it nor patched up his hypothesis; rather, he re-examined the theoretical background of his procedures and in the light of his discoveries reinterpreted Tycho Brahe's observations as confirming an elliptical orbit. Optics - a discipline which Kepler considered an auxiliary science designed to assist in astronomical studies - presented him with practical problems which he did not refrain from attending to. In the few but indicative cases which I have outlined, one can see Kepler grappling with both theoretical and practical problems.

Specifically, Kepler established a correction term for the measurement of the apparent diameter of a celestial body projected in the camera obscura; he sought to transcend subjectivity in the process of taking astronomical observations and yet acknowledged, and indeed catered for, the consequences of the idiosyncrasy of eyesight; and, finally, he had furnished the Galileo telescope with an instrumental theory which, in turn, made it possible to improve upon the design of this telescope and thus to produce a better instrument: more accurate, reliable and, above all, conducive to astronomical observation.

Kepler's wide interest in the problem of error calls for a comprehensive classification which makes distinctions not just between types of observational error, but also between types of error which arise in either the conceptual or the physical, or both aspects of scientific knowledge. In

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1. Rosen, 1965, p.7.

a classification of that kind the theoretical background should be distinguished from the interpretation, and the set-up involved as well as the working of the apparatus should be separated from the actual process of taking the observation or the reading. The failure of the vicarious hypothesis is a case in the category of background theory as distinct from the reinterpretation of the observations of Mars as confirming an elliptical orbit rather than a circular one; furnishing the telescope with an instrumental theory is another, positive, case of the category of background theory. The improved design of the telescope is a positive example of the working of the apparatus. Kepler's attempt to transcend subjectivity in observations, his practical solution which takes care of the idiosyncrasy of eyesight and the correction term for the measurements taken with the camera obscura, are cases which belong to the category of the process of taking observations.

## CHAPTER IV

### A Classification of Types of Experimental Error

#### 4.1 General Discussion

In his book, Mathematical Elements or Natural Philosophy Confirm'd by Experiments, which he intended as an introduction to Newton's natural philosophy, Gravesande (1688-1742) - the propounder of Newtonian physics on the Continent - demands that Nature should be examined 'attentively and incessantly... with indefatigable Pains. That Way', he maintains, 'our Progress will be but slow, but then our Discoveries will be certain';<sup>1</sup> indeed, in his view, even the limits of human understanding could thus be determined.<sup>2</sup> 'What has led most People into Errors,' he observes, 'is an immoderate Desire of Knowledge, and the Shame of confessing our Ignorance. But Reason should get the better of that ill grounded Shame; since,' as he writes - perhaps under the influence of Cusa - 'there is a learned Ignorance that is the Fruit of Knowledge, and which is much preferable to an ignorant Learning'.<sup>3</sup> Although Gravesande admits that many things in nature are hidden from us,<sup>4</sup> he nevertheless holds that 'what is set down in Physics, as a Science, is undoubted'.<sup>5</sup> Thus, notwithstanding his explicit awareness of the limits and faults of human understanding, Gravesande does hold that the results of physics are certain.

According to Gravesande it is the method of mathematical demonstration - a method which the physicist of the scientific revolution employs to deduce, and thus to explain, 'from a few general Principles numberless particular Phenomena or Effects'<sup>6</sup> - that furnishes physics with certainty.

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1. 'sGravesande, 1726, Preface, p.viii. 4. Ibid., pp.xii-xiii.

2. Ibid.

5. Ibid., p.xiii.

3. Ibid.

6. Ibid.

'Whoever will go about that Work any other Way, than by Mathematical Demonstrations, will be sure,' Gravesande warns, 'to fall into Uncertainties at least, if not into Errors'.<sup>1</sup> In his view, this way, the method of mathematical demonstration, is in fact the Newtonian method.

Gravesande is explicit about the trust he invests in the Newtonian method. 'Philosophers,' he observes, 'do not equally agree upon what is to pass for a Law of Nature, and what Method is to be followed in Quest of those Laws.'<sup>2</sup> However, for Gravesande there is no doubt that one ought to follow the Newtonian method. And he maintains that 'whoever will seriously examine, what Foundation this Method of Physics is built upon, will easily discover this to be the only true one, and that all Hypotheses are to be laid aside'.<sup>3</sup>

The great innovation of Gravesande is the attempt to illustrate, as he writes, 'every Thing by Experiments, and to set the very Mathematical Conclusions before the Reader's Eyes by this Method'.<sup>4</sup> Although he admits that 'Mathematicians think Experiments superfluous, where Mathematical Demonstrations will take Place',<sup>5</sup> he has no doubt - since the mathematical conclusions are abstract - that it is easier to grasp these conclusions 'when Experiments set [them] forth... before our Eyes'.<sup>6</sup>

This display of confidence should be contrasted with the cautious

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1. Ibid.

2. Ibid., p.ix.

3. Ibid., p.xvii. Gravesande perceives this method as a two-stage procedure; first there is the deduction of the laws of nature from the phenomena and, second, there is a process of induction to prove the generality of the deduced laws. (Ibid.)

4. Ibid.

5. Ibid., p.xviii.

6. Ibid. Gravesande seems to go here beyond Newton in introducing some Cartesian elements into Newton's methodology. The notion that experiment is a didactic aid has many undertones of the rationalism of Descartes.

advice which Einstein gave the young Heisenberg in the spring of 1926.

'You must appreciate,' Einstein instructed Heisenberg, 'that observation is a very complicated process.'<sup>1</sup> The phenomenon under observation, Einstein explained,

produces certain events in our measuring apparatus. As a result, further processes take place in the apparatus, which eventually and by complicated paths produce sense impressions and help us to fix the effects in our consciousness. Along this whole path—from the phenomenon to its fixation in our consciousness — we must be able to tell how nature functions, must know the natural laws at least in practical terms, before we can claim to have observed anything at all.<sup>2</sup>

In other words, even when we execute this basic requisite of science, namely observation, we ought to be explicitly aware of the assumptions involved. According to Einstein, we must be aware of the fact that while taking observations we assume, even if we are about to formulate a new law of nature which contradicts the old ones, that the current laws — believed to cover the whole path from the phenomenon to our consciousness — function in such a way that we can rely upon them.<sup>3</sup> The possibility that one such law is incorrect, or that the chain which leads from the phenomenon to the observer's consciousness is faulty, should not of course be ruled out.

Although Einstein would presumably have agreed with Gravesande about the power of mathematical demonstrations in physics, he would not have considered the results of these demonstrations certain. Error is a recurring feature of the attempt to test these results in the physical world. As Morris Cohen remarks,

everyone who has ever worked in a laboratory or with instruments of precision knows that the simple laws of nature , so clearly

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1. Heisenberg, 1971, p.63.

2. Ibid.

3. The thrust of Einstein's argument is directed against the view that none but observable magnitudes must go into a physical theory. In Einstein's view, 'it is quite wrong to try founding a theory on observable magnitudes alone... Only theory, that is, knowledge of natural laws, enables us to deduce the underlying phenomena from our sense impressions.' (Ibid.)

formulated in elementary and popular treatises, are never verified with absolute accuracy. The results of actual measurements always differ.<sup>1</sup>

It is standard practice to attribute this lasting discrepancy between the theoretical formulae and the actual measurements not to some faults in the theory but rather to the 'error' of the instruments. Kepler, as I have shown, was successful in his scientific quests precisely because he thought that errors cast doubt on his results - he suspected that they did not pertain only to the instruments - and searched indefatigably to account for them. But in any event, the refinement and improvement of the available instruments never eliminate the discrepancy between theory and experiment. 'On the contrary,' Cohen concludes, 'it often compels us to abandon the simple law in favour of a more complicated one.'<sup>2</sup>

Yet, there is a prevalent belief that exact agreement can be attained. Jeffreys suggests that the use of the expression 'exact science' and the scarcity of explicit statements in popular works on physics concerning the imperfection of agreement between physical laws and observation, have contributed to this belief. Where errors of observation are mentioned at all they are dismissed, in Jeffreys' words, 'as a minor complication'.<sup>3</sup>

1. M.R. Cohen, 1949, p.224.

2. Ibid., Cf., Duhem, 1974, pp.168-179.

3. Jeffreys, 1973, p.63. Jeffreys observes that the usual presentation in textbooks of physics starts by stating a set of laws, taken as fundamental, and proceeds to develop a series of consequences - the observational evidence being only incidental. This approach is unsatisfactory and indeed misleading since it takes no account of errors; if the results were compared with observation directly, almost every law would be rejected. As Jeffreys remarks, 'the observed value hardly ever agrees exactly with prediction, and the laws are taught as if they were exact... [this treatment] is essentially inapplicable unless it is supplemented by the notion of error'. (Ibid., p.80.) In Jeffreys' view 'the laws of physics, so far as they relate to observations, become statements of probability distributions. The quantities of physics arise fundamentally as parameters in these probability distributions.' (Ibid.) The principal objective of Jeffreys' treatment is to show that 'errors of observation have to be considered in the process of establishment of the laws: we can, if we like, say that there is a form of the law that expresses exact relations between true values, but the law that is verified is a modification of this that takes account of probabilities of errors of observation in different ranges'. (Ibid., p.212.)



Sellars' casual remark that 'once [errors of measurement and other forms of experimental error]... have been discounted, our attention can turn to the logico-mathematical structure',<sup>1</sup> epitomizes this attitude towards the problem of error. Indeed, with a few exceptions,<sup>2</sup> the problem of error has been treated as quite incidental to the pursuits of science: 'a tiresome but trivial excrescence on the neat deductive structure of science'.<sup>3</sup>

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1. Supra, motto.

2. Chwistek, 1948. According to Chwistek, measurement is a crudely defined activity. Since slight differences in the results of a measurement are disregarded, a measurement can give no basis for the establishment of a one to one correspondence between its results and a real number. In other words, as he writes, 'many real numbers correspond to one measurement and the class of real numbers is not precisely determined. The only way to make this statement more precise is to fix the limits between which the number obtained by measurement can vary, i.e. to designate two numbers, between which the number obtained by measurement may be found'. (Ibid., p.256.) The thrust of Chwistek's criticism is that 'the concept of ideal length is meaningless and that experience furnishes not determinate numbers but classes of numbers which lie between certain limits'. (Ibid., p.257.) As a consequence, Chwistek recommends the adaptation of concepts such as determinism and indeterminism to this state of affairs. (Ibid.) Jeffreys points out that there are three fundamental misconceptions which pervade most modern accounts of scientific principles and are the principal source of confusion. 'The first is that in some sense scientific laws are statements made with certainty. The second is that physical measures can be exact. The third is that there is a clearly marked boundary between science and ordinary thought,' (Jeffreys, 1973, p.183.) According to Jeffreys, the second fallacy arises from ignoring the occurrences of observational errors. Jeffreys' general objective is to show how the first two fallacies are avoided by the theory of probability.
3. Mellor, 1967, p.6. Scriven's view is a subtle example of this attitude; while acknowledging that laws of nature are virtually all known to be in error, he concludes that since 'they represent great truths... we forgive them their errors'. (Scriven, 1961, pp.91, 101.) Mellor is in fact a critic of this attitude. In a series of papers published in *Philosophy of Science* (Mellor, 1965; 1966; 1967), he 'tried to show how the effects of experimental error or imprecision can be accommodated within a testable, deductive scientific structure'. (Mellor, 1965, p.121.) There is now a growing interest in the actual practice of the scientific method in general and the method of experimentation in particular; see for example the works of N. Cartwright, A. Franklin, I. Hacking and A. Pickering. As Pickering demands, 'experimental techniques, methods, and procedures - the very stuff of empirical science - should not be treated as unproblematic adjuncts of some higher theoretical exercise'. (Pickering, 1981, p.235.)

Jeffreys emphatically states that 'exact agreement between physical laws and observation was never attained',<sup>1</sup> and he criticizes the exaggerated attention which the uncertainty principle has attracted. Although 'the uncertainties treated in the quantum theory are far smaller than any of the discrepancies between previous theories and observational results... these [discrepancies]', Jeffreys remarks, 'had attracted little attention from philosophers'.<sup>2</sup> J.O. Wisdom indeed claims that 'all the standard philosophies

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1. Jeffreys, 1973, p.183.

2. Ibid., pp.63-4. Evidently, the attraction of the uncertainty principle is due to the very fact that it is indeed a principle; a fundamental limitation on the accuracy with which one can execute a measurement, independent of one's experimental skill. There is however a phenomenon which is associated with classical physics that sets a definite limit to the ultimate sensitivity of measuring instruments beyond which one cannot advance, namely Brownian motion. (Ising, 1926; Barnes and Silverman, 1934.) To be sure, classical physics is based on an idealization which can be valid only under the assumption that absolute accuracy is within reach; that is, that mathematical accuracy can be attained with respect to the initial state of the physical system involved. In his paper, 'The Decline and Fall of Causality', F. Waismann takes stock of this classical attitude. He remarks that 'the accuracy of any measurement, it was supposed - rather light-heartedly as it would seem now - could be increased to any degree by improved technique. Irrespective of whether absolute precision will ever be attainable with our blunt instruments, we can at any rate go on refining our measuring methods, it was assumed, and proportionally our predictions concerning the future will become more and more reliable. That there is no limit to this approach, this was, ultimately, the tacit assumption underlying classical physics, and one so brilliantly vindicated by the successes in astronomy'. (Waismann, 1959, p.107.) Waismann associates the decline of causality with the recognition of 'how utterly Utopian the idea of absolute precision is'; (Ibid., p.112.) and the fall of causality with the discovery of the uncertainty principle. (Ibid.) However, according to Chwistek the uncertainty principle amounts to a relation between different types of measurement; it did not yield something essentially new with respect to the problem of determinism. (Chwistek, 1948, p.257.) Chwistek would presumably have agreed with Born's claim that 'statements like "A quantity  $x$  has a completely definitive value" (expressed by a real number and represented by a point in the mathematical continuum) seem... to have no physical meaning'. (Born, 1956, p.167.) From the axiomatic standpoint, it appeared to A. Ruark undesirable that the differential character of the principal equations of physics implies that physical systems are governed by laws which operate with a precision beyond the limits of verification by experiment. He thus recommends the use of the calculus of finite differences rather than the differential method, since no real physical intervals ever become strictly infinitesimal. (Ruark, 1931.)

In Chwistek's view this proposal may be misleading. As he remarks, 'the physical quantity  $dx/dt$  is unquestionably a fiction since it

of science that have been current in this century fail to solve the problem of the data-theory gap.<sup>1</sup>

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Cont.Fn.2 from p.149.

does not occur in any physical process. However, the quotient  $(x_2 - x_1)/(t_2 - t_1)$  is also a fiction because just as nature does not perform the operation of the differential calculus it does not perform arithmetical operations. The velocity  $(x_2 - x_1)/(t_2 - t_1)$  is an expression equally as good or bad as the expression  $dx/dt$ . What is important is to know how to use both of these expressions. If it is supposed that physical phenomena and time are both discontinuous, the derivatives would still remain meaningful.' (Chwistek, 1948, p.240.) Addressing himself to the same issue, E.W. Adams has sought to modify the fundamental assumption that an observation can yield the conclusion that the measure of an object on a given dimension is equal to such and such a value (which differs from the 'true' value by some error). His modification consists in dropping the assumption that equality (in measure) is observable, and upholding the assumption that inequality (greater or lesser) can be observed. (Adams, 1965.) That view echoes Cusa's demand that we should 'ascend to [the recognition] that truth, freed from material [conditions], sees, as in a definition, the equality which we cannot at all experience in things, since in things equality is present only defectively'. (Cusa, 1981, p.88 (Bk.II, Ch.1).) In his study of mathematical magnitude and experiment, Poincaré suggests that the formula:  $A=B$ ,  $B=C$ ,  $A < C$ , 'may be regarded as the formula of the physical continuum'. (Poincaré, 1952, p.22.) He then remarks that 'here is an intolerable disagreement with the law of contradiction, and the necessity of banishing this disagreement has compelled us to invent the mathematical continuum... Although we may use the most delicate methods, the rough results of our experiments will always present the characters of the physical continuum with the contradiction which is inherent in it'. (Ibid., pp.22-23.) Körner concurs with this view (Körner, 1963-1964, p.276); indeed, for him a 'deductive unification of an empirical field by means of classical logic is not a mere ordering but an idealisation'. (Ibid., p.282.) Körner finds it necessary to develop a logic which can deal with the inexact concepts of the empirical realm. (Körner, 1963-1964; 1966.) He further shows how the distinction between the empirical and the theoretical discourse is relevant to the question of the nature of pure and applied mathematics. (Körner, 1960, Ch.8.) However, in Mellor's view for 'operationally defined continuous variables, the inexactness of experience can be captured within a deductive structure by the exact concepts of operational and conceptual imprecision'. (Mellor, 1965, p.111.) Körner's 'provisional' logic may become largely superfluous, if, as Mellor argues, the effects of experimental error, or imprecisions, are 'recognized and allowed for, and not dismissed as an unfortunate triviality'. (Ibid.; Mellor, 1967, p.9.)

1. Wisdom, 1971, p.281. Wisdom argues that 'the fundamental assumption that sets the problem for... all three approaches [instrumentalism, conventionalism and classical induction], is the givenness of observations; observations are simply there, as the empiricist tradition has it, waiting to be recorded by that classical camera obscura, the human mind. In other words, they share a philosophy of observationalism'. (Ibid.) On the gap between mathematical accuracy and physical approximation see also Duhem, 1974, pp.132-143. Cf., Mellor, 1965, pp.110-111.

However, the experimenter constantly encounters these discrepancies between theories and observational results; indeed, part of his daily routine consists, as M. Polanyi intimates, in explaining away these discrepancies.

'In my laboratory,' Polanyi writes, 'I find the laws of nature formally contradicted at every hour, but I explain this away by the assumption of experimental error. I know that this may cause me one day to explain away a fundamentally new phenomenon and to miss a great discovery. Such things have often happened in the history of science. Yet I shall continue to explain away my odd results, for if every anomaly observed in my laboratory were taken at its face value, research would instantly degenerate into a wild-goose chase after imaginary fundamental novelties.'<sup>1</sup>

The process of explaining away discrepancies is therefore vital to the continuation of proper experimental research.

Focusing our attention on these discrepancies we see that their origins can be of different kinds: occurring in different contexts and arising from different causes. To clarify this complex array of different causes of discrepancy it is useful to have, as a heuristic device, a system of classification of experimental error.

Any attempt at a classification presupposes a principle, a criterion, which warrants a juxtaposition of the objects intended for classification; indeed, such a criterion makes the classification meaningful. It has been a common fallacy to hold that the criteria which underlie a system of classification exhibit the true structure of the diverse phenomena collated in the system; thus, amidst diversity unity was claimed to be attained. As the representation of the true structure of the phenomena, a classification frequently assumed the definite article to become the classification; that is, the unique representation of the concerned phenomena. There is no need to dwell here upon this fallacy; suffice it to say that unity may

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1. Polanyi, 1964, p.31. Polanyi concludes that 'just as there is no proof of a proposition in natural science which cannot conceivably turn out to be incomplete, so also there is no refutation which cannot conceivably turn out to have been unfounded. There is a residue of personal judgement required in deciding - as the scientist eventually must - what weight to attach to any particular set of evidence in regard to the validity of a particular proposition'. (Ibid.)

be achieved, but it is a conceptual unity. Had a different set of criteria been applied to the phenomena, different structures would have been reflected and more 'unities' attained. Indeed, different classificatory systems for the same phenomena lead to the invention of different classificatory terms. To classify is in fact to produce an artifice<sup>1</sup> which is intended to illuminate the phenomena under discussion - in this case, experimental errors - by acknowledging diversities and recognizing similarities.

There have been some objections to the enterprise of classifying errors. Notwithstanding the long tradition of classifying fallacies,<sup>2</sup> de Morgan disapproves of such an enterprise. In his book, Formal Logic, he opens the chapter on fallacies with the categorical statement that 'there is no such thing as a classification of the ways in which men may arrive at an error: it is much to be doubted', de Morgan asserts, 'whether there ever can be'.<sup>3</sup> In his view the aim of a discussion on fallacies is merely 'the production of examples to exercise a beginner in the detection of breaches of rule'.<sup>4</sup> Nevertheless, he acknowledges that it is very desirable to draw the reader's attention to the problem of identifying clearly the mistake, and he proceeds to analyse fallacies on the basis of his own classification.<sup>5</sup>

As the analogy of probability to logic is sufficiently close, Venn finds it advisable - in his book, The Logic of Chance - to adopt the traditional plan which works of logic exhibit; that is, to devote a chapter to the description and classification of the different ways in which the rules

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1. Cassirer remarked that 'all systems of classification are artificial. Nature as such contains individual and diversified phenomena. If we subsume these phenomena under class concepts and general laws, we do not describe facts of nature'. (Cassirer, 1962, p.209.)
2. See, for example, Hamblin, 1972.
3. De Morgan, 1847, p.237.
4. Ibid.
5. Ibid., Ch.XIII (On Fallacies).

of logic may be transgressed. He thus proposes 'to collect a few of the errors that occur most frequently, and as far as possible to trace them to their sources'.<sup>1</sup> However, he is reluctant to offer a classification of errors, claiming that 'it will be hardly worth the trouble to attempt any regular system of arrangement and classification'.<sup>2</sup>

Half a century after de Morgan had expressed his criticism, H.W.B. Joseph formulated this attitude of disapproval in a similar fashion: 'Truth may have its norms, but error,' Joseph states, 'is infinite in its aberrations, and they cannot be digested in any classification'.<sup>3</sup> Nevertheless, Joseph defended the insertion of a chapter on fallacies partly because tradition was in its favour; but mainly because 'familiarity with some of the commonest types of fallacy... may help us to avoid them, by helping us more readily to perceive them'.<sup>4</sup> In Joseph's view, 'it is a great abridgement of criticism to be able to name the types (of fallacies), and refer a particular fallacy to one of them'.<sup>5</sup> Thus, Joseph acknowledged that a classification of fallacies could after all facilitate the analysis of error.

He notes, however, that 'the same inconclusive argument may often be referred at will to this or that head of fallacies'.<sup>6</sup> Moreover, 'it may be doubted... if the types of error can be exhaustively detailed, and the classification completed'.<sup>7</sup> It appears that central to Joseph's criticism is the view that such a classification can never be definite nor can it

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1. Venn, 1962, p.332 (Ch.XIV (Fallacies)).

2. Ibid.

3. Joseph, 1906, p.528.

4. Ibid., pp.526-27.

5. Ibid., p.527.

6. Ibid., p.528.

7. Ibid., p.529.

be complete.<sup>1</sup> He thus remarks that the task of classifying fallacies 'is one which does not admit of fully satisfactory performance. Still,' he continues, 'it can be better and worse done', and he asks, 'what classification of fallacies are we to adopt?'<sup>2</sup> Joseph then proceeded to develop his analysis of fallacies using the traditional Aristotelian scheme.<sup>3</sup>

It is worth noting further that Joseph distinguished between error and fallacy; a distinction which is close to my view of error and mistake.

'If the falsity of the premiss can only be ascertained empirically,' then, according to Joseph, 'there is error,... [but] if... the falsity of the premiss is to be ascertained by thinking out the consequences of certain relations, or conceptions, in the circumstances of a given case, then,' Joseph holds, 'we are guilty of fallacy, or defect of reasoning, in overlooking it.'<sup>4</sup>

Evidently, error may arise in the study of nature since it involves more than reasoning.

'It required,' as Joseph remarks, 'the use of the senses in observation, the recording of facts, the formation of conceptions, or hypothesis, the invention of a nomenclature, etc. There are obstacles in the way of the successful performance of these operations, no less than of reasoning.'<sup>5</sup>

Clearly, there is a parallel between classifying fallacies and classifying errors; a parallel to which Bacon, it may be recalled, explicitly referred in his undertaking.<sup>6</sup> Joseph thus concludes that 'the fallacies of the common Logic waylay us in the work of reasoning. His [Bacon's] idola arise from circumstances that waylay us in all those tasks'.<sup>7</sup>

Joseph considered Bacon's classification an illustration in another field of the difficulties which render a perfect classification of fallacies impracticable. In his view 'the division [of the idola] was not logically

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1. Ibid., pp.529-32. 'The type, if one may say so, is fluid.' (Ibid., p.532.)

2. Ibid., p.532.

3. Ibid., Ch.XXVII (Appendix on Fallacies).

4. Ibid., p.532.

5. Ibid., p.544 footnote no.1.

6. Supra, Ch.I, p.16 footnote no. 7.

7. Joseph, op.cit.

perfect, and the enumeration in each group is doubtless not complete'.<sup>1</sup>

To be sure, a classification is an artifice which by its very nature cannot be unique, nor can it be, in this particular instance, exhaustive. However, a classification which is general and yet consists of clear distinctions may shed light on the phenomena under discussion. Joseph's criticism may well be right and indeed in no way do I claim that a perfect classification of experimental error is practicable; but it remains to be seen whether or not a classification of experimental error can be attained which illuminates pitfalls that beset the method of experimentation.

The most common classification of experimental error is the classification which distinguishes between two categories of error: systematic and random errors. As we shall see, this is not the kind of classification needed for our purposes.

Systematic errors, as their name indicates, systematically obstruct the measurement from obtaining the intended actual value by shifting the result - either positively or negatively - by a magnitude which may be constant or may vary in some regular fashion. Random errors, in contrast, are disordered in their incidence and vary accidentally in their magnitude. A simple example of the former is an incorrect calibration of the measuring instrument; undue mechanical vibrations of the equipment - vibrations which interfere with the measurement - constitute a cause of the latter.<sup>2</sup>

The distinction between the terms accuracy and precision - terms which are mistakenly used interchangeably - corresponds to this dichotomy between systematic and random errors. Accuracy refers to the closeness of the measurements to the 'true' value of the sought physical quantity, whereas precision indicates the closeness with which the measurements agree with one another independently of their relations to the 'true' value. Accuracy

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1. Ibid.

2. See, for example, Topping, 1975, p.10; Parratt, 1961, pp.64-69.



thus implies precision but the converse is not necessarily true.<sup>1</sup> It is therefore better to be almost accurate rather than precisely wrong.

In scientific and technical writing it is common to find different usages of the word error. One school of thought considers error the difference between the experimental result and the 'true' value; another usage is that error is the number placed in the statement of the result after the plus and minus sign, irrespective of the 'true' value.<sup>2</sup> It has been recently suggested - in a Code of Practice addressed to the National Physical Laboratory - to use the term uncertainty to cover this multiple usage of the word error.<sup>3</sup> In this Code of Practice the uncertainty of a measurement is divided into two categories: the random uncertainty and the systematic uncertainty.<sup>4</sup> According to this Code, the estimation of random uncertainty is derived by a statistical analysis of repeated measurements while the estimation of systematic uncertainty is assessed by non-statistical methods and much depends on the judgement of the experimenter in allocating limits to this uncertainty.<sup>5</sup>

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1. Topping, *ibid.*, p.14; Parratt, *ibid.* Cf., Franklin, 1981a, p.367 footnote no.1.
  2. Campion, Burns and Williams, 1980, p.25. Campion is presently the Deputy Director of NPL. E.R. Cohen and J.W.M. DuMond observe in their 1965 review of the fundamental constants that there seem to be two completely incompatible ways in which experimenters regard experimental error. 'Some regard the number following the sign of ambiguity as expressing "limits of error" with the unstated implication that the true value lies anywhere within the gap and that there is something rather virtuous in overestimating the magnitude of this gap "for safety" or "to take care of possible but unknown sources of systematic error"... (S)uch an error estimate... is not a quantitative estimate, but a statement of inequality: the error is less than or equal to so-and-so much'. The other approach - an approach which in Cohen's and DuMond's view is far more useful - is 'to regard the number following the plus-or-minus sign as an estimate of the width parameter of some statistical distribution of observed values which would be obtained if the measurement were replicated a number of times'. (Cohen and DuMond, 1965, p.540.) Cohen and DuMond in fact deplore the former approach. (*Ibid.*, p.541.)
  3. Campion et al., *ibid.*, pp.1, 30.
  4. *Ibid.*, p.1.
  5. *Ibid.*, pp.14-15, 23.

Le Comité International des Poids et Mesures pointed out, in 1981, that the terms 'random' and 'systematic uncertainty' are often used in ambiguous and inconsistent ways. It therefore adopted a recommendation which suggests, in part, to replace these terms with Category A uncertainty: an uncertainty evaluated by statistical means, and Category B uncertainty: an uncertainty evaluated by other means with an element of 'subjective appreciation'.<sup>1</sup> From a different perspective, Jeffreys has defined systematic errors as 'errors that could be precisely calculated for each observation, given the values of certain parameters'.<sup>2</sup> Evidently, random errors do not have this feature. However, it appears that, whatever the terms and the definitions, the methods used for estimating uncertainties constitute the underlying criteria of this classification.

Many textbooks and manuals on the design of experiments and the treatment of data can attest to the general acceptance of this classification which is sometimes even considered exhaustive. In the introduction to his book, Statistical Treatment of Experimental Data, H.D. Young remarks that there is however an additional class of errors - a third class - which contains, as he puts it, 'what are sometimes called errors, but which are not, properly speaking, errors at all. These include,' Young specifies, 'mistakes in recording numbers, blunders of reading instruments correctly, and mistakes in arithmetic'.<sup>3</sup> But according to Young, 'these types of inaccuracies have no place in a well-done experiment. They can always be eliminated

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1. Recommendation CI-1981. See Henrion and Fischhoff, 1984, p.25 footnote no.9. I am grateful to Professor A. Franklin for bringing this paper to my attention.
  2. Jeffreys, 1973, p.77 (my emphasis).
  3. Young, 1962, p.3. (At the time of publication, Young was Assistant Professor of Physics, Head of the Department of Natural Sciences, Carnegie Institute of Technology.)

completely by careful work'.<sup>1</sup> However, in his view, the same applies to systematic errors; as he maintains, 'an experimenter whose skill has come through long experience can consistently detect systematic errors and prevent or correct them'.<sup>2</sup> Thus, a skilled experimenter is expected to eliminate completely the errors contained in the third class - they are not, as Young maintains, errors at all - and prevent or correct those of the first class. The experimenter will face just one group of errors, namely, those of the second class: random errors.

Worsnop and Flint, to take another example, also characterize the persistence of random errors - they call them accidental errors: their law of action being unknown<sup>3</sup> - in contrast to the possible elimination of systematic errors once their source is detected and the rule governing them is known. In their influential book, Advanced Practical Physics for Students, which has seen many editions since its first publication in 1923, they presuppose the possible elimination of all systematic errors. Worsnop and Flint claim that it is found that when systematic errors - errors which arise, as they specify, from instruments, external conditions such as temperature variation, or from idiosyncrasies - are eliminated, there is still a margin of error which requires further consideration. This margin of error, they assert, is due to accidental errors.<sup>4</sup> The claim that in order

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1. Ibid. Parratt describes these faults as outright mistakes and calls them blunders. (Parratt, 1961, p.69.) Thomson and Tait assign these faults to a third class: the class of what they call avoidable mistakes. (Thomson and Tait, 1872, pp.112-13.) Worsnop and Flint also call errors of this type mistakes. They use the term mistake to denote a fault of measurement or of observation which can be avoided by care on the part of the observer. Thus, they remark that 'inexperienced observers or observers not in a normal state [sic] make errors of varying magnitude which should strictly be described as mistakes'. (Worsnop and Flint, 1951, pp.3-4.) Evidently the personal qualification of the experimenter is at the root of this usage of the term mistake. This usage does not therefore correspond to the distinction between mistake and error put forward in the Introduction of this thesis.
  2. Young, *ibid.*, p.2.
  3. Worsnop and Flint, 1951, p.5.
  4. Ibid., p.4.

to perceive the occurrence of accidental errors one has to conceive of the possibility of eliminating all systematic errors, does not befit, in my view, an advanced practical physics book.<sup>1</sup>

Naturally, Young has some advice to offer the skilled experimenter concerning the inevitable occurrence of random errors. He informs him that 'it is found empirically that... random errors are frequently distributed according to a simple law. This makes it possible to use statistical methods to deal with random errors'.<sup>2</sup> And sure enough, 'this statistical treatment', Young announces, 'will form the principal body of the following discussion [that is, Young's treatise]'.<sup>3</sup>

Thus, Young's view is that, apart from random errors, all experimental errors can be eliminated, and that the distribution of random errors can be captured by a simple law which, he claims, has been established empirically.

However, in practice it is very rarely, if ever, the case that the experimenter can remove the systematic errors altogether.<sup>4</sup> Furthermore, the distribution of errors follows the normal law only approximately, even

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1. Worsnop and Flint distinguish further between constant and systematic errors. In their view, 'constant errors are those which affect the results of a series of experiments by the same amount. An example is the case of the faulty graduation of a scale'. (Ibid., p.4.) They maintain that these errors are distinct from systematic errors which 'occur according to some definite rule, such as would be the case in readings on a circular scale if the pointer were not pivoted at the centre'. (Ibid.) However, this is not a categorical distinction; in fact according to these definitions, a constant error is a particular instance of systematic error.
  2. Young, 1962, p.3.
  3. Ibid. Cramér expresses this attitude in these words: 'Obviously they [the systematic errors] will not lend themselves to probabilistic treatment, and accordingly we shall assume in the sequel that we are dealing with observations which are free from systematic errors'. (Cramér, 1966, p.229.)
  4. In Duhem's view the estimation of the degree of exactness of an experiment requires, among other things, that 'we evaluate the systematic errors that could not be corrected; but, after making as complete an enumeration as possible of the causes of these errors, we are sure to omit infinitely more than have been enumerated, for the complexity of concrete reality is beyond us'. (Duhem, 1974, p.162.)

when the quantity to be measured is as steady as possible.<sup>1</sup> Although this approximation can be justified under much wider conditions, namely the central limit theorem, it is still the case, as Jeffreys remarks, that these conditions 'are seldom known to be true in actual applications. Consequently,' Jeffreys concludes, 'the normal law as applied to actual observations can be justified, in the last resort, only by comparison with the observations themselves'.<sup>2</sup> But in any event, Jeffreys writes that he will proceed 'as if the normal law held'.<sup>3</sup>

This state of affairs has led to much confusion with respect to the validity of the normal law; a confusion to which Lippmann wittily referred in his remark to Poincaré: 'Everybody believes in the exponential law of errors: the experimenters, because they think it can be proved by mathematics; and the mathematicians, because they believe it has been established by observation.'<sup>4</sup> Clearly, Young belongs to the latter camp. In the NPL's Code of Practice, Campion et al. state quite clearly that 'there is of course no reason for experimental observations to follow the Normal distribution exactly - it is a convenient mathematical expression which fits most of the experimental observations'.<sup>5</sup> They stress that 'it should be recognized that this is an assumption which may not always be justified'.<sup>6</sup>

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1. Jeffreys, 1973, p.67.

2. Ibid., emphasis in the original. Needless to say, this is a vicious circle; it is the result of justifying the treatment of observations by exclusively referring to observations. The appeal to probability is an attempt to break this vicious circle. Cf., Cohen and DuMond, 1965, pp.540-41.

3. Jeffreys, *ibid.*, p.68.

4. Quoted by Whittaker and Robinson, 1924, p.179. (See Poincaré, Calcul des prob., p.149.)

5. Campion et al., 1980, p.11.

6. Ibid., p.5. Margenau critically remarks that 'experience presents the scientist with innumerable skew distributions, differing perceptibly from the normal law. These he often dismisses or corrects, because for some hitherto unstated reason he objects to them. He uses the normal distribution both as an inductive generalization from experience and as a criterion for the trustworthiness of that experience. Thus he is lifting himself by his bootstraps unless an independent argument can be given for the normalcy of that distribution'. (Margenau, 1950, p.114.)

Although the statistical treatment which Young advocates stands or falls on the ground of the validity or otherwise of the distribution law, he does not raise this difficulty. Like many other writers in this field,<sup>1</sup> Young avoids the general problem of experimental error and, using statistical methods, addresses himself to the problem of random errors. It appears that his task is to furnish the experimenter with a mathematical tool which can compress through a process of abstraction the scattered results the experimenter has obtained, into one value bounded by upper and lower limits.

Two objections which are closely connected may be raised against this classification. Firstly, as the protagonists of this classification are interested in the resultant error and not so much in its source, they are bound to classify together phenomena which may indeed perpetrate, as it were, the same kind of error, e.g., random error, but are nevertheless distinct as to their causes. Thus, for example, Young classifies together 'small errors in judgement on the part of the observer, such as in estimating tenths of the smallest scale division',<sup>2</sup> with 'unpredictable fluctuations in conditions, such as temperature,... or mechanical vibrations of the equipment'.<sup>3</sup> The implication of this arrangement is that errors which

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1. Works in this field can be generally divided into two categories: those that focus on the statistical treatment of data and those that are primarily concerned with the physics and practice of the experimental method. A sample of the former can be found in the bibliography of Topping's book. (Topping, 1975, p.116.) The latter category contains works such as Threlfall's On Laboratory Arts, Strong's Modern Physical Laboratory Practice and Braddick's The Physics of Experimental Method, not to mention that of Worsnop and Flint. Threlfall and Strong do not discuss the problem of experimental error in its generality as a relevant issue to the method of experimentation. (Strong however has a specific discussion of errors which may occur in the employment of Geiger counters.) Braddick, on his part, updates these works and amongst other things adds a section on the reduction of observations and on the statistical analysis of errors. (Threlfall, 1898; Strong et al., 1940; Braddick, 1963.) Kohlrausch's pioneering textbook appears to be exceptional in being explicit about the variety of errors which may occur in taking measurements, although he amplifies the role of the mathematical theory of error at the expense of elaborating the errors themselves. (Kohlrausch, 1894.)
  2. Young, 1962, pp.2-3.
  3. Ibid., p.3.

are peculiar to an individual observer are conflated with errors that have originated in the instrument. Worsnop and Flint, on their part, place errors which arise from instruments and external conditions such as temperature variation on an equal footing with those arising from personal idiosyncrasies, since all of these errors are in their view systematic.<sup>1</sup>

Grouping together such diverse sources of error obscures the nature of experimental error. To put it differently, the dichotomy between systematic and random errors does not focus on the source of the error; rather, it examines its nature by applying a mathematical criterion which judges whether it is systematic or random.

The fact that the criterion which underlies this dichotomy is mathematical constitutes the second objection. Most of the writers in this field satisfy themselves with a few remarks concerning the origin and treatment of systematic errors. Having briefly dealt with systematic errors, these authors proceed into a detailed analysis of the theory of error which provides the mathematical tool for the treatment of random errors. However, under this treatment the error which we know to originate in a certain physical condition is transformed into a technical term which is defined mathematically as either the difference between the result and the 'true' value or as the departure from the mean - the mean being another mathematical concept designed to capture the plurality of the results. In this sense, errors are in effect residuals, as indeed Jeffreys calls them.<sup>2</sup>

The upshot of such analysis is that the experimenter gets a mathematical insight into his collection of data. As Cramér remarks, the object of the theory of error 'is to work out methods for estimating the numerical values of the required magnitudes by means of a given set of observations, and also to make it possible for the observer to arrive at some conclusion

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1. Supra, p.158.

2. Jeffreys, 1973, p.64.

with respect to the degree of precision of the estimates obtained'.<sup>1</sup> In accordance with that theory the experimenter identifies the unknown 'true' value of the observed magnitude with the mean of the corresponding normal distribution, and the degree of precision with the standard deviation of the distribution.<sup>2</sup> However, the experimenter remains in the dark as to the physical and conceptual circumstances in which the errors have originated.<sup>3</sup>

To illuminate these aspects we shall treat experimental error as a twofold phenomenon consisting of physical and conceptual elements, and not as a mathematical abstraction. Indeed, it appears that these two elements are invariably interwoven: there is always a certain physical condition and the experimenter's conceptual understanding of it. Most scientists and philosophers of science regard error as an essentially probabilistic

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1. Cramér, 1966, p.229.
  2. Ibid., p.230. In view of the fact that the theory is somewhat arbitrary, it is no surprise that there is no general agreement among scientists as to the adequacy of this measure of precision and that some other measures are being used (e.g., probable error). In their 1965 review of the fundamental constants, Cohen and DuMond state their preference for expressing all of their error measures in terms of standard deviations, that is to say, root-mean-square deviations from the mean, rather than in terms of probable errors. In their view 'it is a grave mistake... to regard the error expressed in standard deviations as simply differing by a multiplying factor (1.48) from the probable error. The probable error is defined as that measure of dispersion such that the odds are even that the observation may lie either inside or outside the given limits. Only for the Gaussian distribution are the standard deviation and the probable error related to each other by the numerical factor just cited. They are two entirely different kinds of dispersion measures, and the standard deviation is far more general in its applicability under the theory of least squares'. (Cohen and DuMond, 1965, p.541.) Campion et al. note in their Code of Practice that 'the standard deviation depends only on the precision of the technique and apparatus used and, so long as these are not changed, it will not change significantly however many observations are taken; on the other hand, the standard error of the mean depends on the number of observations in the sample as well as on the precision of the technique. The two quantities... present different information', they conclude and recommend that 'the statement of both in a result may sometimes be appropriate'. (Campion et al., 1980, p.7.) Commenting on the mathematical theory of error, Margenau remarks that 'the philosopher of science is obliged to take note of this remarkable fact: both "truth" and "tolerance" must be fished out of the uncertainties of the immediately given by more or less arbitrary rules not immediately presented in Nature'. (Margenau, 1950, p.113. )
  3. Cf. Duhem, 1974, p.162.



phenomenon resulting from some stochastic process.<sup>1</sup> Although probabilistic considerations are of considerable importance in coping with inaccuracy and imprecision, we are concerned here with error as an epistemological concept, and this view calls for a different classification of experimental error.<sup>2</sup>

To arrive at a classification which focuses on the epistemological nature of experimental error, one has to distinguish between the different contexts in which error may arise and within each context to determine the kind of possible reasons for its occurrence.

Distinguishing different stages in the method of experimentation, we may classify errors as arising in:

1. Background theory;
2. Assumptions concerning the actual set-up and its working;
3. Observational reports; and,
4. Theoretical conclusions.

#### 4.2 Background theory

Any attempt to create artificially certain physical conditions with a view to recording a particular phenomenon, indeed, even the making of the simplest observation, takes place within a theoretical framework which underpins the experimental inquiry and thus determines and directs it.

This theoretical framework – the 'background theory' – is tentatively taken

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1. W.C. Salmon, for example, holds that the concept of error is basically probabilistic. (Salmon, 1967, p.65.)
  2. Historical studies of experimental error as a probabilistic concept have been carried out by L. Tilling, 1973, and O.B. Sheynin. Sheynin published a series of papers on this subject in the Archive for History of Exact Sciences (see in particular his general historical essay, 'Mathematical Treatment of Astronomical Observations', 1973). A complete bibliography of his study can be found in Sheynin, 1983; see also Sheynin, 1966.

for granted and considered correct. The experiment, or, for that matter, the observation, is not designed to test or demonstrate the background theory; rather, the experimenter relies upon this theory to advance the argument which he then puts to the test. The background theory, to paraphrase Wittgenstein, is not so much the point of departure, as it is the element in which arguments have their life.<sup>1</sup> However, the theoretical framework cannot be immunized, so to speak, against errors and thus must occupy one domain in the present scheme.

One may clarify this point by making the simple observation of looking at a family photograph. Having examined such a photograph, one instantly recognizes the images which comprise it and is able to relate them in a very confident fashion to the relatives one has come to know. It seems that such an exercise does not require the slightest effort: there is no need for the observer to consider the mechanical, optical and chemical theories which underlie the production of such an artefact. However, one can take this exercise to its extremes and consider an electron-micrograph and a radio-photograph transmitted, say, by a spacecraft such as Mariner; the theory in the background which was previously set aside must come now to the fore. Since one cannot be sure about the relation between the unfamiliar composition of images to the so-called reality this composition is supposed to represent, one has to rely upon the background theory to ensure that the composition is indeed a faithful representation.<sup>2</sup>

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1. 'All testing, all confirmation and disconfirmation of a hypothesis takes place already within a system. And this system is not a more or less arbitrary and doubtful point of departure for all our arguments: no, it belongs to the essence of what we call an argument. The system is not so much the point of departure, as the element in which arguments have their life.' (Wittgenstein, 1977, p.16e (§105).)
  2. Many difficulties may be raised with regard to the usage of photographs in scientific inquiries; prominent amongst them is the possibility of creating artefacts which do not represent faithfully the objects under study (e.g., problems in enhancement techniques). See also Hillman and Sartory, 1977. For a positive view concerning electron-micrographs see Hacking, 1983, pp.186-209 Ch.11 (Microscopes).

Clearly, we can further distinguish within this class between the general theory and the instrumental theories which are assumed to govern the performance of the instruments intended for use in the experiment. Amongst these instrumental theories there is one theory which deserves a special attention, namely, the theory which underlies the set-up itself. This theory occupies therefore a subclass of its own in this category of background theory. It is in this primary stage of the experiment that the experimenter develops the theory of the set-up, plans its design and specifies the initial conditions.

The experimental method can be generally characterized as a method which inquires into the nature of the phenomena by varying a certain group of elements or a single one, and recording the change, if any, in some other elements. In fact, as Mach remarks, 'what we can learn from an experiment resides wholly and solely in the dependence or independence of the elements or conditions of a phenomenon.'<sup>1</sup> In studying a particular agent or cause, the experimenter should seek to isolate the object of his study. Thus, the design must be such as to increase the effects due to the object of study until these effects exceed considerably the unavoidable concomitants; the latter can then be considered as only disturbing these effects, and not essentially modifying them.<sup>2</sup> In other words, the experimenter has to make the set-up most sensitive to the object of study and as insensitive as possible to all other elements that may play a part. As Weyl notes, 'this accounts, among other things, for the tedious efforts involved in screening off all kinds of "sources of error".'<sup>3</sup> Evidently, the attainment of isolation is essential for achieving this goal. Indeed, the mark of a great experimenter is the ability to simplify the set-up in such a way

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1. Mach, 1976, p.149 Ch.XII (Physical experiment and its leading features).

2. Thomson and Tait, 1872, p.108.

3. Weyl, 1963, p.153.

that only the factor in question remains in evidence while all other influences become negligible.<sup>1</sup>

In the background-theory stage of the experiment error may arise as a consequence of using false theory. Corresponding to the distinctions between the general theory, the instrumental theories and the theory of the set-up, we may determine one possible source of error as the falsity of one or more of these theories. It should be stressed, however, that a false theory does not necessarily lead to an error in the final experimental result. One may develop the argument of an experiment within the framework of a false general theory and yet produce a correct result; one may use an instrument or even the apparatus itself without a proper theoretical understanding of its working, indeed its theory may be wrong or even missing altogether, and yet the overall result still be correct.

In addition to falsity as a source of error, error may occur in this stage when the theory is incomplete, or when it is correct in the main but false in some isolated aspect - a falsity which does not undermine the theory itself. Furthermore, in cases where the theory makes use of some external - either calculated or measured - constants, error may arise if these auxiliary elements are in error.

A case in which use was made of a falsely calculated constant is related by Wood. Mach reported that in a treatise, published in 1865, entitled Homes Without Hands, Wood relates the following episode.

Maraldi had been struck with the great regularity of the cells of the honeycomb. He measured the angles of the... rhombs, that form the terminal walls of the cells, and found them to be respectively  $109^{\circ}28'$  and  $70^{\circ}32'$ . Réaumur, convinced that these angles were in some way connected with the economy of the cells, requested the mathematician König to calculate the form of a hexagonal prism terminated by a pyramid composed of three equal... rhombs, which would give the greatest amount of space with a given amount of material. The answer was, that the angles should be  $109^{\circ}26'$  and  $70^{\circ}34'$ . The difference, accordingly, was two minutes. Maclaurin, dissatisfied with this agreement, repeated Maraldi's measurements, found them correct,

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1. Mach, 1976, p.151.

and discovered, in going over the calculation, an error<sup>1</sup> in the logarithmic table employed by König. Not the bees, but the mathematicians were wrong, and the bees had helped to detect the error!<sup>2</sup>

Mach went on to comment that

any one who is acquainted with the method of measuring crystals and has seen the cell of a honeycomb, with its rough and non-reflective surfaces, will question whether the measurement of such cells can be executed with a probable error of only two minutes.<sup>3</sup>

A clear example of an auxiliary measured constant which introduced an error into the final experimental result is E. Harrington's value for the viscosity of air, of which R.A. Millikan made use in his evaluation of the charge of the electron. Millikan gave much weight to Harrington's result - Harrington was a colleague of Millikan - but the result was vitiated by several serious systematic errors.<sup>4</sup> Harrington's value was too low by about 0.4% and consequently Millikan's value of  $e$  was too low by about 0.6%.<sup>5</sup> As E.R. Cohen et al. observe, this error 'remained completely unsuspected for a period of about 15 years [1916-1931]'.<sup>6</sup>

An illustration of an incomplete theory which resulted in a discrepancy between theory and observation is the discrepancy between Newton's theoretical value for the speed of sound in air and actual measurement of that speed. T.S. Kuhn points out that 'this discrepancy, about twenty per cent, had

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1. It would be a mistake in my terminology.
  2. Mach, 1974, p.548. Cf., Fleck, 1979, pp.32-33.
  3. Mach, *ibid.*, p.549.
  4. E.R. Cohen, K.M. Crowe and J.W.M. DuMond, 1957, p.116. In Millikan's view, Harrington succeeded in making a determination of the viscosity of air which is 'altogether unique in its reliability and precision. I give to it alone', Millikan writes, 'greater weight than to all the other work of the past fifty years in this field taken together. For the individual determinations, though made with different suspensions and in such a way as to eliminate all constant sources of error save the dimensions of the cylinders, never differ among themselves by as much as .1 per cent, and the error in the final mean can scarcely be more than one part in 2000'. (Millikan, 1917, p.9.)
  5. In the evaluation of the charge  $e$ , the coefficient of viscosity enters to the  $3/2$  power.
  6. Cohen, Crowe and DuMond, *op.cit.*

been one of the scandals of physical science for more than a century and had repeatedly though fruitlessly drawn the attention of Europe's outstanding theoretical scientists, including Euler and Lagrange'.<sup>1</sup> When Laplace suggested around 1802 that heating by compression might account for this discrepancy, direct confirmation was not possible. The existing thermometers had such a large heat capacity that their slow response did not permit direct measurements. Competing some ten years later for the French Academy's prize concerning this problem, Delaroche and Bérard did measure the heat capacity of air at two quite different pressures. From these measurements Laplace was able to derive the first good theoretical value for the speed of sound in air. By 1825, Laplace succeeded in reconciling experiment and theory by using a new value for the speed of sound in addition to more direct data of adiabatic compression.<sup>2</sup> However, as Kuhn remarks, 'in retrospect, the agreement was artificial... [I]n this case, as in remarkably many others throughout this period, errors of theory and experiment compensated more than well enough to satisfy expectation'.<sup>3</sup>

Michelson's measurement of the velocity of light is an example of a correct set-up theory which contains an isolated mistake. In his 1941 review of the various determinations of the velocity of light in vacuum (c), Birge reported that

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1. Kuhn, 1958, pp.136-37.
  2. Ibid., pp.136-39. Specifically, Laplace showed that if  $\gamma$  is defined as the ratio of the heat capacity of a gas at constant pressure to its capacity at constant volume, then the speed of sound must have Newton's value multiplied by the square root of  $\gamma$ .
  3. Ibid., pp.137-38. Kuhn remarks further that in the first half of the nineteenth century 'the heat capacity of calorimeter and thermometer was usually far larger than that of the gas they contained. As a result, few empirical bench marks were available to those who developed the caloric theory of gases, and it was difficult to tell which of the available and by no means consistent experimental measurements was reliable... The selection and evaluation of empirical tests was as much a matter of taste and judgement as the selection and evaluation of theory'. (Ibid., p.140.) Cf., Truesdell, 1980; Fox, 1971.

Michelson's published result of 299796 km./sec. is... in need of revision. To obtain it he used 67 km./sec. for the correction to vacuum from air at an assumed average temperature of 20°C. and at an average barometric pressure of 625 mm. Hg. It is easy to show that his correction results from the use of the wave index of refraction, instead of the correct group index of refraction.<sup>1</sup>

Birge remarked that 'this is one of the most inexplicable errors that I have ever come across in the literature'.<sup>2</sup> According to Birge, Michelson never applied the concept of group velocity in his work on the absolute value of  $c$ ; indeed, 'Michelson did not even apply group velocity to his observed index for water. If he had, it would have spoiled the apparent agreement'.<sup>3</sup> Although Michelson was aware of the distinction between group and wave velocity,<sup>4</sup> he did not apply it correctly.

Ehrenhaft's measurements of the charge of the electron which 'demonstrated' the existence of subelectrons, provides an illustration for a false set-up theory that resulted in an error. Like Millikan, Ehrenhaft presupposed Stokes' law to govern the fall of the particles. However, he used metal particles which were much smaller than the oil drops of Millikan, not to mention their irregular surface in contrast to the smooth surface of the droplets. Consequently, Stokes' law was not applicable to the physical conditions Ehrenhaft had created, and his final result was in error.<sup>5</sup>

An interesting case of a false background theory which actually prevented the execution of what may be considered a correct idea for an experiment, occurs in the history of the experimental research into the phenomenon of light-pressure. In a paper delivered to the French Academy in 1731, J.J. Dortous de Mairan rightly remarked that all direct experiments which had been designed to detect this phenomenon were bedevilled by effects

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1. Birge, 1941, p.93.

2. Ibid.

3. Ibid., p.94.

4. Ibid., pp.93-94.

5. This case will be taken up as a case-study.

which the heat produced by the light had caused.<sup>1</sup> Nevertheless, he did construct a sensitive pivoted mill of low friction which was extremely mobile, and trained on it the focus of a lens; but he still could not establish the phenomenon of light-pressure unambiguously. Indeed, as the light was focused on one of the vanes, the 'machine turned sometimes to one side, sometimes to the other'.<sup>2</sup> Mairan concluded that these results were due to the heating of the air around the machine. Naturally, the next step would have been to perform the same experiment in a vacuum. Mairan was explicitly aware of this possibility, but refrained from carrying it out, deciding that he 'need not give [himself] the trouble'.<sup>3</sup> He was of the opinion that, as he writes, 'there is in our atmosphere amongst that more gross air which we breathe and which does not at all penetrate glass, another more subtle air or some other fluid which penetrates glass'.<sup>4</sup> In Mairan's view, this more subtle air would lead, in these light-pressure experiments, to the same uncertain results; he therefore did not pursue further the idea of executing this experiment in vacuo.<sup>5</sup>

The theory of the set-up may mislead in giving the impression that it governs different set-ups of the same type. A case in point are the experiments on the double scattering of electrons in the 1920s and thereafter. As Franklin and Howson point out,

the electrons used in these experiments came from both  $\beta$ -decay and thermionic sources and physicists of the day believed that the type of source used made no essential difference. Later work, however,

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1. Worrall, 1982, p.142.

2. Quoted by Worrall, *ibid.*, p.143.

3. *Ibid.*

4. *Ibid.*

5. Worrall remarks that 'Mairan seems here illicitly to be assuming, independently of experiment, that light does indeed exert a pressure - a fact in which, as a corpuscularist, he firmly believed. For, had he reperformed the experiment in vacuo and found the erstwhile movement quelled then it would follow that this "more subtle air" could play no role in the phenomenon'. (*Ibid.*)



showed that  $\beta$ -decay electrons were longitudinally polarized while thermionic electrons were unpolarized, which resulted in significantly different experimental results.<sup>1</sup>

However, theories can be irrelevant to the success of the experiment. Galileo, for example, did not have any theory to explicate the working of his telescope.<sup>2</sup> Another example is the case of the Wassermann reaction. In his book, Genesis and Development of a Scientific Fact, Fleck demonstrates how the experimental discovery of the relation between the Wassermann reaction and syphilis arose from false assumptions and irreproducible initial experiments, as well as many errors and detours. This case illustrates, among other things, how a false theory can underlie a great experimental discovery.<sup>3</sup>

#### 4.3 Assumptions concerning the actual set-up and its working

Whereas the background-theory class focuses on the theoretical basis of an experiment, the second class is concerned with the actual process of setting up and operating the apparatus involved, that is, the hardware. One may characterize this second stage as the materialization of the theoretical requirements stipulated in the first stage; in other words, the nuts and bolts stage.

In the process of constructing and setting up the various hardware required by the experiment, the experimenter makes numerous assumptions. The most common of these is the assumption that the parts one has procured and their arrangement stand up to the required specifications and conditions. To be sure, it is possible to ascertain physically some of these assumptions by putting them to the test; however, since such tests involve further measurements, one has here a regressive sequence of measurements that for

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1. Franklin and Howson, 1984, pp.60-61. Cf. Franklin, 1979, pp.247-48.

2. Feyerabend, 1979, Chs.9-11.

3. Fleck, 1979.

practical reasons must be truncated. Moreover, the experimenter has to ensure that the specifications are maintained while the experiment is carried out, or to allow for any change that may occur. Arising therefore in this class is the general type of error which originates in the belief that the set-up has met all the requirements, including the specified initial conditions, which, however, in point of fact it has not: the vacuum may be poor, the electric and magnetic fields may not be as uniform as expected, the insulation may not suffice, convection current or diffusion may occur, and so forth; the experimental result may thus mislead.

The determination of the Hall effect which is of great interest in the theory of liquid metals abounds with such difficulties. An electric current is passed through a liquid metal (at a temperature of, say, 1000°C) in a strong transverse magnetic field. A very small voltage has then to be measured across the specimen. One can easily imagine the problems of containing the sample, maintaining it at a uniform temperature, avoiding convection currents and effects of magneto-hydrodynamic voltages, damping down vibrations, amplifying the signal, and so forth. As Ziman remarks, 'it is scarcely surprising that a decade of experimental work by several very accomplished research workers has not produced an agreed set of data for this (in principle) basic and elementary physical parameter'.<sup>1</sup>

J.J. Thomson successfully showed in 1899 that the entities emitted in interactions of light with metal - including neutral metal plates - were the very same entities which made up the cathode rays.<sup>2</sup> This result firmly confirmed the assumption that the photoelectric effect was due to interaction of light with the atomic or subatomic constituents of the metal surface. It thus provided a definite framework within which work on this effect could be carried on. Further experimental work gave

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1. Ziman, 1978, p.59.

2. J.J. Thomson, 1899; Humphreys, 1968, p.46.

J.J. Thomson the impression that increase in temperature intensified the photoelectric action; the emission velocities of the ejected electrons appeared to be dependent on temperature. He therefore argued that the photoelectrons must be the free electrons of the metal as these particles partake in the energies of thermal agitation.<sup>1</sup>

However, in 1907 Millikan and Winchester demonstrated conclusively that this result of J.J. Thomson, namely, that temperature affects the emission velocities of photoelectrons, was based on misleading experimental data: data drawn from experiments conducted in air, rather than in a vacuum. Millikan and Winchester could not find in a careful survey of eleven metals any dependence of the photoelectric effect on temperature. This finding, coupled with Lenard's discovery that the emission velocities of photoelectrons are independent of the intensity of the incident light,<sup>2</sup> led Millikan and Winchester to conclude that their result 'constitutes very conclusive evidence that, if free electrons exist at all within metals, it is not these electrons which escape under the influence of ultra-violet light'.<sup>3</sup> In other words, J.J. Thomson's result was in error.

N. de Lacaille (1713-1762), the meticulous French astronomer who mapped the southern skies, was troubled by the fact that the result of his geodetic survey in the vicinity of Cape Town supported the hypothesis that the earth is a prolate, not an oblate, spheroid. Although he partially rechecked the result, he could find no error and it remained a puzzle for some years. Apparently the result was due to the deviation of the plumb line at his southern station caused by the large mass of Table Mountain.<sup>4</sup>

In their 1965 review of the fundamental constants, Cohen and DuMond

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1. J.J. Thomson, 1902; Humphreys, *ibid.*, pp.47-48.

2. Lenard, 1900; 1902.

3. Millikan and Winchester, 1907, p.198; Humphreys, 1968, p.48.

4. Gingerich, 1973,b, p.544. I am grateful to Professor B.R. Goldstein for suggesting this case.

point out that there is a  $17 \text{ km sec}^{-1}$  difference between the general region of values obtained for  $c$  and the older result adopted in 1941 by R.T. Birge. The case of the  $17 \text{ km sec}^{-1}$  discrepancy between the old Birge-recommended value of  $c$  and the newer values obtained by more modern methods of measurements has been described by Birge as one of the most astonishing systematic errors in the history of physics. Cohen and DuMond remark that

the chief source of error in the older estimate came from a systematic error in the experimental result of Michelson, Pease, and Pearson, performed in a mile-long evacuated tube laid on unstable soil near Santa Ana, California. Although some 2885 replicated observations of the time of flight were made, there were only a few (2 or 3) determinations of the distance. All the time measurements were made at night and the distances in the daylight. The site was near the ocean and variations<sub>1</sub> in the results which seems correlated with the tides were observed.

Yet, no one knows for sure what caused the systematic error in this work.

#### 4.4 Observational reports

The preceding two stages constitute the foundation - in the abstract and the concrete sense - upon which the empirical programme can be brought to its completion, that is, knowledge through sense perception. The central feature of the third stage is thus the process of observation. It is at this stage that the contact, as it were, between the senses and the allegedly isolated 'world' of the experiment is explicitly made. This contact may be at fault due to either misinterpretation of what has been otherwise correctly observed, or the limitation of the senses and their subjective nature. I call the former possible fault observational error, and the latter - personal equation.

An observational error occurs when the observer makes a correct observation but fails to bring into consideration the external circumstances

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1. E.R. Cohen and J.W.M. DuMond, 1965, p.550.

in which the observation took place; such a failure arises from misinterpreting the correct observation. These errors usually occur when the observer encounters a misleading situation of which he is not aware, and proceeds to interpret his otherwise correct observation without allowing for the misleading circumstances.

Were the experimenter to make a pointer reading, for example, and should he ignore the gap between the pointer and the scale, his reading might be in error due to the phenomenon of parallax. Another case in point is aberration: if the observer were to disregard his own motion, he would not succeed in locating the actual position of the object he is observing. However accurate his observation may be, he would inevitably determine the apparent position of the object concerned. In my terminology it would be an observational error, if the observer were to consider the result of this observation the true position of the object.

In the subclass of personal equation I subsume, in contrast to observational errors, those errors which originate in the limitation of the senses and their subjective nature. It was F.W. Bessel (1784-1846) - the famous mathematician and astronomer, the pioneer in the more exact measurements of modern astronomy - who conceived the notion of personal equation. Working in a period of great interest in errors of astronomical observation and their mathematical treatment,<sup>1</sup> Bessel perceived, in 1816, the importance of the Kinnebrooke incident, and published an analysis of the case in 1822 - a publication which attracted immediate attention.

The Kinnebrooke incident took place at Greenwich Observatory where, in January, 1796, the Astronomer Royal, N. Maskelyne (1732-1811), dismissed his assistant, Kinnebrooke, for observing the times of stellar transits almost a second later than he himself had done. It should be stressed

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1. By 1816, both Laplace and Gauss had already published their seminal works on the mathematical treatment of errors of observation.

that the error was considered serious, since the calibration of the Greenwich clock depended upon such observations.<sup>1</sup>

This event was recorded in the pages of the journal, Astronomical Observations at Greenwich,<sup>2</sup> and might have passed into oblivion had it not been for Bessel who saw its significance. Having studied the case, Bessel set himself the task of determining whether or not such a difference could be found amongst observers with more experience than Kinnebrooke. To his astonishment he found great differences which he presented in the form of the equation:  $A-B=x$  sec. The difference between observer A and observer B was called the 'personal equation'.<sup>3</sup> It was thus realized that even the most expert observers must allow themselves to be corrected by statistical averages from other observers. As R.L. Gregory points out, 'for the first time in science, the average took over to correct the individual testimony of expert observers'.<sup>4</sup> However, Bessel went further

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1. Maskelyne followed the Bradley 'eye and ear' method which combines the audible beats of the pendulum clock with the perceptible transit of a given star across the hair-line marking the meridian. In this method, as E.G. Boring reports, 'the observer looked at the clock, noted the time to a second, began counting seconds with the heard beats of the clock, watched the star cross the field of the telescope, noted and "fixed in mind" its position at the beat of the clock just before it came to the critical wire, noted its position at the next beat after it had crossed the wire, estimated the place of the wire between the two positions in tenths of the total distance between the positions, and added these tenths of a second to the time in seconds that he had counted for the beat before the wire was reached. It is obviously a complex judgment', Boring remarks. 'Not only does it involve a coordination between the eye and the ear, but it requires a spatial judgment dependent upon a fixed position (the wire), an actual but instantaneous position of a moving object, and a remembered position no longer actual. Nevertheless, "the excellent method of Bradley" was accepted and regarded as accurate to one or at least two tenths of a second. In the face of this belief', Boring concludes, 'Kinnebrooke's error of eight tenths of a second was a gross error and justified Maskelyne's conclusion that he had fallen "into some irregular and confused method of his own" and his consequent dismissal'. (Boring, 1950, p.135. Cf., Gregory, 1984, p.212.)
  2. Maskelyne, 1799.
  3. For Bessel's discovery, see Bessel, 1823; 1826; 1836; 1876. Cf., Boring, 1950, pp.136-37, 150 (Ch.8 (The Personal Equation)); Gregory, 1984, pp.210-16.
  4. Gregory, *ibid.*, p.213.

and, by comparing directly and indirectly his observations with those of Struve, showed that one could not reliably 'calibrate the observer' - the 'personal equation' being itself variable.<sup>1</sup> Yet, the variability of the 'personal equation' was not so great as to render correction entirely useless. Bessel, for example, always observed in advance of Struve by an amount varying between 0.770 and 1.021 seconds.<sup>2</sup>

During the fifty years since Bessel's discovery of the phenomenon of 'personal equation', astronomers were much concerned with this problem and sought methods of correction and elimination. As it turned out, the emerging new discipline of experimental psychology gradually took hold of this issue, focusing its attention on explanation rather than on correction or elimination. Hermann von Helmholtz's (1821-1894) measurement of the velocity of the nervous impulse in 1850, which demonstrated that nervous conduction is considerably slower than the speed of sound in air,<sup>3</sup> and J. Hartmann's (1814-1876) experiment in 1858, which showed expectation and anticipation to be decisive elements of the 'personal equation',<sup>4</sup> gave much impetus to the study of this problem from physiological and psychological standpoints respectively. Furthermore, the development of the electromagnetic

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1. Boring, 1950, p.137.

2. Ibid., p.138. Duhem remarked that 'astronomers try to determine this information [the acuteness of the observer's senses] in the mathematical form of a personal equation, but this equation partakes very little of the serene constancy of geometry, for it is at the mercy of a splitting headache or painful indigestion'. (Duhem, 1974, p.162.)

3. Helmholtz's values are 42.9 and 25.0 metres per second; they lie well within the range as determined by modern methods. Recent determinations give values for the velocity of the nervous impulse as high as 120 metres per second for the largest nerve fibres and as low as 1 metre per second for the smallest. For comparison, the speed of sound in air is about 330 metres per second. (Boring, 1950, pp.48, 144.)

4. Hartmann's contribution was very important since it demonstrated that the 'personal equation' is not simply a variable delay physiologically explained, but that it also depends upon psychological elements. (Ibid., p.145; Gregory, 1984, pp.215-16.)

circuit made at that time the construction of chronographs practicable, and it became possible to determine what is called the 'absolute personal equation': the reaction time in making a movement as rapidly as possible after perceiving a signal with respect to a standard time measure, and not relative to the reaction time of another observer.<sup>1</sup>

The sources of error of the personal-equation type reside therefore either in the physiological condition of the observer's senses, or in his mental state or both. Examples of the former are colour blindness and slow reaction time, and of the latter - anticipation and hallucination.

The distinction between observational errors and errors of the personal-equation type reflects the categorical difference between causes from without and causes from within. External circumstances can give rise to the possibility of committing an observational error, whereas one's own physiological condition and psychological make-up determine one's personal equation.

An historical case which clearly demonstrates the problem of psychological expectation and the limitation of the senses - both possible sources of error pertain to the subclass of personal equation - is the case of the canals of Mars. There is no item in the entire history of the telescopic observations of Mars which has been more widely proclaimed, more vehemently debated, and more abruptly forgotten than the canals.

The first astronomer to observe this phenomenon, Schiaparelli of Milan, noted in 1877 that 'there are on this planet, traversing the continents, long dark lines which may be designated as canali... Their arrangement appears to be invariable and permanent'.<sup>2</sup>

The possibility that the intersecting dark lines which Schiaparelli had observed were literally canals, built by intelligent beings, was immediately taken up; it gave weight to the belief that there exists a highly

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1. Boring, *ibid.*, pp.140-141, 147; Gregory, *ibid.*, p.215.

2. Quoted by Mutch et al., 1976, p.21.



developed civilization on Mars. One of the most energetic proponents of this theory was the American P. Lowell (1855-1916), who argued that the 'intelligent creatures' of Mars were living on an ageing, desert-like, planet whose water was trapped in the polar caps; hence the canals: an artificial device to transport water into the equatorial zones.<sup>1</sup>

Lowell admitted that it is very difficult to see the canals.

'Success,' he wrote, 'depends upon the acuteness of the observer's eye and upon the persistence with which he watches for the best moments in the steadier air.'<sup>2</sup>

Thus, he continued, 'not everybody can see these delicate features at first sight, even when pointed out to them; and to perceive their more minute details takes a trained as well as an acute eye, observing under the best conditions. When so viewed, however, the disk of the planet takes on a most singular appearance. It looks as if it had been cobwebbed all over.'<sup>3</sup>

Since it appeared that the whole planet was encompassed by the canals, Lowell suggested that the Martian community could act as a unit throughout its globe; he therefore argued that the community on Mars had an intelligent and non-bellicose character.<sup>4</sup>

However, the critics argued that even an unbiased human eye would connect, under conditions of poor resolution, discontinuous blotches and streaks into regular straight lines; and, secondly, since the effective resolution of the best telescopes is about 100 km. on the Martian surface, Lowell's claim for identifying canals of 50 km. and less in width is simply not compatible with optical principles. The critics did not deny the visual effect of dark markings on the image of Mars; they objected, however, to identifying them, as narrow continuous lines, with actual Martian surface markings.<sup>5</sup>

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1. Ibid.

2. Quoted by Mutch et al., *ibid.*, p.23.

3. Ibid. A typical observational report on the canals of Mars can be found in *Nature*, 84(1910)172-3.

4. Mutch et al., *ibid.*, pp.23-24.

5. Ibid., p.24.

As late as 1962, Lowell Observatory published a photographic atlas of Mars, claiming to provide 'each reader with the best possible opportunity to distinguish the fine lines and to judge their reality [sic] for himself'.<sup>1</sup>

When the Mariner pictures arrived, it became clear that the critics' point of view has been vindicated. The pictures obtained by the high resolution cameras aboard the spacecraft made it amply clear that no regular, linear elements of the size required by the canals theory were observable. It appears that poor resolution initiated a condition which facilitated a visual synthesis of discontinuous elements; the interpretation of these elements as canals was supported in turn by the belief that there is a Martian community. Thus, the observation of the canals was in error due to physical-physiological limitation as well as psychological expectation.<sup>2</sup>

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#### 4.5 Theoretical conclusions

Having carried out the observations and the concomitant measurements, the experimenter arrives at the final stage of his study. To conclude the enquiry, that is, to obtain the final result, the experimenter analyses his collection of data, and then incorporates the result of his analysis with an external theory so that the result can be put to use in either substantiating or refuting a theoretical claim, or laying altogether a new foundation. This is the goal of the experiment.

Two processes are therefore involved in this final stage: firstly, there is the process of reduction, of analysing the data with a view to obtaining a coherent final result; and, secondly, the process of spelling out the physical meaning of this result.

It is in the first process that the mathematical theory of error is

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1. Quoted by Mutch et al., *ibid.*

2. *Ibid.*, p.25.

employed; indeed, it is only in the process of reduction that the mathematical procedures for treating errors come to the fore. And since the distinction between systematic and random errors is central to this analysis of error, it is here that this dichotomy is directly applicable. However, when the experimenter does not apply the mathematical procedures of the theory of error correctly, then the result is not an error but rather a mistake. The theory of error is ultimately a set of rules and as such its misapplication results in a mistake in the sense I have propounded. Yet, error may still arise due to the inapplicability of the distribution of random errors presupposed in the mathematical analysis. Furthermore, error may occur in the process of assessing systematic errors - a process for which no clear set of rules exists.

A fault in the second process, that is, the process of rendering the final result physically meaningful, may be due to two distinct causes. Such a fault may originate in either misunderstanding the physical result the experimenter has obtained, or reasoning fallaciously. In view of my terminology, the former fault is an error, an error of interpretation, whereas the latter is a mistake. In this subclass there are therefore sources of error and mistake which are located in the faculties of understanding and reasoning, respectively.

On the night of the 13th of March, 1781, William Herschel noticed in the course of a search for star pairs, a curious object which appeared to be in motion relative to nearby stars. He recorded in his notes that he had seen 'a curious either nebulous star or perhaps a comet'.<sup>1</sup> A few nights later he stated that what he had found 'is a comet, for it has changed its place'.<sup>2</sup> The possibility that the object was a planet - an interpretation which was compatible with the visual evidence - did not occur to Herschel initially. On the 26th of April, Herschel reported his discovery to the

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1. Quoted by Grosser, 1979, p.19.

2. Ibid.

Royal Society in a paper entitled 'Account of a Comet'.<sup>1</sup>

Notified of this discovery, astronomers attempted to determine the motion of the new body. Several attempts were made to compute a suitable parabolic (cometary) orbit as soon as a few observations had been accumulated. However, it was quickly realized that each set of elements coincided with the observed positions for a few days and then rapidly diverged. The wasted efforts were due to one single reason: an uncritical acceptance of Herschel's interpretation that the object was a comet. It was in the summer of that year that Herschel's interpretation was rejected and the object he had discovered was reinterpreted as a planet: the planet Uranus.<sup>2</sup>

The few observations of Uranus did not suffice for accurate computations of its orbit. Seeking more observations, it occurred to Bode that Uranus might have been considered a star in possible earlier sightings. He therefore scrutinized old astronomical catalogues with a view to finding Uranus being recorded as a star. He was indeed rewarded; he found two such sightings: in 1690 by Flamsteed and in 1756 by Mayer. In the following years sixteen more 'preplanetary' observations of Uranus were discovered.<sup>3</sup>

The planet Neptune was sighted for the first time as a planet by Galle who, on the basis of the predicted elements computed by Leverrier, searched for it in the right region in September, 1846. However, Neptune had in fact been sighted earlier, in 1795, by Lalande, but he considered it a star. An examination of Lalande's manuscript showed that he had made two observations of the planet, on the 8th and 10th of May; finding them discordant, he had rejected one as probably in error, and marked the other as questionable. It is not unlikely that a re-examination of the region to see which observation was in error would have led Lalande to the discovery of the planet more than half a century before it was actually recognized.

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1. Ibid.

2. Ibid., pp.20-21.

3. Ibid., pp.24-26, 40-41.

Indeed, the difference between the positions of these two sightings is given accurately by the elements of Neptune's orbit.<sup>1</sup>

Another case in point is what Franklin calls the 'nondiscovery of parity nonconservation'.<sup>2</sup> In the late 1920s a group of American physicists obtained an asymmetry in the double scattering of electrons. Today this effect is routinely interpreted as a manifestation of parity violation. However, at the time it was inconceivable that parity violation might occur; it was therefore assumed that something had gone wrong with the apparatus.<sup>3</sup>

A subtle case of error of interpretation occurred in the experimental confirmation of Einstein's equation of the photoelectric effect. In his presentation speech of Einstein's Nobel Prize, Arrhenius made it clear that the Nobel Committee for Physics had chosen Einstein especially for his contributions to quantum theory: his studies of specific heat and the photoelectric effect.<sup>4</sup> Arrhenius declared in his speech that

Einstein's law of the photo-electrical effect has been extremely rigorously tested by the American Millikan and his pupils and passed the test brilliantly. Owing to these studies by Einstein the quantum theory has been perfected to a high degree.<sup>5</sup>

A year later, in 1923, when Millikan was awarded the Nobel Prize 'for his work on the elementary charge of electricity and on the photoelectric effect,'<sup>6</sup> the chairman of the Nobel Committee expressed the view that if Millikan's studies of the photoelectric effect had given a different result, the law of Einstein would have been without value; the chairman pointed

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1. Ibid., pp.115-17, 139-40. Cf., Polanyi, 1964, pp.90-91; Humphreys, 1968, pp.34-42.
  2. Franklin, 1979.
  3. Franklin has carried out a thorough study of this case, *ibid.*; cf., Pickering, 1982, p.441.
  4. Nobel Lectures, 1967, p.479.
  5. Ibid., p.480. On the deliberation of the Nobel Committee see Pais, 1983, pp.502-512.
  6. Nobel Lectures, 1965, p.49.

out that Einstein received the Prize after Millikan had confirmed the law experimentally.<sup>1</sup>

The results of Millikan's painstaking experimental work on the photoelectric effect<sup>2</sup> did indeed establish the validity of Einstein's equation, and, moreover, provided an accurate determination of  $h$ . However, Millikan categorically rejected Einstein's hypothesis of a light corpuscle of energy  $h\nu$ . In his concluding paper on this work, Millikan opined that Einstein had put forward 'the bold, not to say the reckless, hypothesis of an electromagnetic light corpuscle of energy  $h\nu$ '.<sup>3</sup> Millikan considered this hypothesis 'reckless' since, as he explained,

an electromagnetic disturbance which remains localized in space seems a violation of the very conception of an electromagnetic disturbance, and second because it flies in the face of the thoroughly established facts of interference.<sup>4</sup>

Although his experiments confirmed Einstein's equation for the photoelectric effect, Millikan felt strongly that 'the semi-corpuscular theory by which Einstein arrived at his equation seems at present to be wholly untenable'.<sup>5</sup>

Indeed, despite the

'complete success of the Einstein equation, the physical theory of which it was designed to be the symbolic expression is found,' according to Millikan, 'so untenable that Einstein himself, I believe, no longer holds to it.'<sup>6</sup>

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1. Ibid., p.53.

2. Millikan described the experimental arrangement as 'a machine shop in vacuo'. (Millikan, 1916,b, p.361.)

3. Ibid., p.355.

4. Ibid.

5. Ibid., p.383.

6. Ibid., p.384. Pais traced this belief of Millikan to a remark Einstein had made in 1911, at the first Solvay congress. 'I insist,' Einstein said, 'on the provisional character of this concept (light-quanta) which does not seem reconcilable with the experimentally verified consequences of the wave theory.' (Quoted by Pais, 1983, p.383.)

By putting a different interpretation on Einstein's confirmed equation,<sup>1</sup> Millikan found himself in a peculiar situation; as he remarked,

we are in the position of having built a very perfect structure and then knocked out entirely the underpinning without causing the building to fall. It stands complete and apparently well tested, but without any visible means of support. These supports must obviously exist, and the most fascinating problem of modern physics is to find them. Experiment has outrun theory, or, better, guided by erroneous theory, it has discovered relationships which seem to be of the greatest interest and importance, but the reasons for them are as yet not at all understood.<sup>2</sup>

The scepticism concerning Einstein's light quantum hypothesis prevailed till about 1924. It was the discovery of the Compton effect that provided, together with the photoelectric effect, that 'interlocking theoretical and experimental matrix',<sup>3</sup> from which a concept such as the photon derives its validity.

In these cases the experimental and, for that matter, the observational data were correct and indeed significant. However, according to current theories the final results were in error due to erroneous interpretations of the data.

In this classification of experimental error we have distinguished and ordered - according to chronological sequence - four different stages in the method of experimentation. We have identified in each stage - a

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1. Millikan conceded that 'the photoelectric effect..., however it is interpreted, if only it is correctly described by Einstein's equation, furnishes a proof which is quite independent of the facts of black-body radiation of the correctness of the fundamental assumption of the quantum theory, namely, the assumption of a discontinuous or explosive emission of the energy absorbed by the electronic constituents of atoms from ether waves. It materializes, so to speak, the quantity "h" discovered by Planck through the study of black-body radiation and gives us a confidence inspired by no other type of phenomenon that the primary physical conception underlying Planck's work corresponds to reality'. (Millikan, 1916,b, p.385.) However, Millikan went on to develop a substitute for Einstein's theory based largely on Planck's theory. (Ibid., pp.385-88; Millikan, 1922, pp.231-38.) An overview of the various interpretations of this effect can be found in Humphreys, 1968, pp.43-59; cf., Stuewer, 1970.
  2. Millikan, 1922, p.230.
  3. Stuewer, 1970, p.263.

stage being in itself a category in the classification - possible sources of mistake and error that may impair the final result. The heuristic value of this classification - that is, the potentiality of this classification to elucidate experimental errors - can be now illustrated by applying the classification to actual experiments in the history of science, particularly in physics.

However, before embarking upon the exposition and analysis of the case-studies, it is important to note that underlying the distinctions embedded in the proposed classification there exist some outstanding philosophical issues. In the first place it should be stressed that the logical chain-like appearance of the method of experimentation as portrayed by the classification does not reflect the actual way in which an experiment is executed. To be sure, as N.R. Hanson points out, 'experiments are designed to be as chain-like as possible'.<sup>1</sup> However, as he continues, to characterize an experiment as "this happens, then that, then those things take place, which results in..." is a bad caricature. The balance, timing ingenuity and planning involved in a first-class experiment... can be represented in links-in-a-chain fashion only by a casual observer.'<sup>2</sup> Although the logical presentation of an experiment seems to require a 'one-way artery of traffic',<sup>3</sup> we know that this is not the way the method of experimentation is conducted; rather, the experimenter moves forward and backward along the four stages we have distinguished as if no clear and distinct categories exist.

Yet, the critic of the method of experimentation - the philosopher of science who looks for possible faults in this method - must attain an external vantage point from which he can exercise a critical outlook. Distinguishing categories in this method and identifying in each of them

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1. Hanson, 1979, p.67 (emphasis in original).

2. Ibid.

3. Watson, 1938, p.117.



typical sources of mistake and error is one way of applying a critical outlook.

However, the critic has to be aware of the various philosophical assumptions which underlie the attempt to establish these categories. For example, the distinction between the category of background theory and the observational-reports category is based on the assumption that a report of observation can be dissociated from any theoretical consideration: that, in principle, it is possible to have a description of a state of affairs which is not bound by any theory. This assumption is however very problematic; indeed, it has been challenged by many students of philosophy of science. Hanson, for example, writes that 'there is a sense... in which seeing is a "theory-laden" undertaking. Observation of  $x$  is shaped by prior knowledge of  $x$ '.<sup>1</sup> According to this view, each observational report necessarily contains an interpretation of what is seen; it is thus laden with a theory about the nature and explanation of what is seen.

Richard L. Gregory, the student of perception who subscribes to the views of Hanson, I. Lakatos and T.S. Kuhn in so far as they hold that observations and experimental tests are highly theory-laden, is of the opinion that 'a great deal has to be accepted before observations can be made at all'.<sup>2</sup> According to Gregory,

the fact that vision is usually sufficient for immediate object identification distracts us from realizing the immense importance of contextual knowledge for reading data from signals. Scientific data from instruments are almost always presented with explicit collateral information on how the instrument was used, what source it was directed to, its calibration corrections and scale settings.<sup>3</sup>

Gregory's main thesis is that perceptions are like scientific hypotheses; he thus draws a parallel between the instruments of science and the sense organs, and indeed between the procedures of science and processes carried out by the brain to generate perceptions.<sup>4</sup>

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1. Hanson, 1979, p.19.

2. Gregory, 1984, p.255.

3. Ibid., p.397.

4. Ibid., p.395.

P. Feyerabend has argued from a different angle that the distinction between observational terms and theoretical terms is defunct. Although experience, as Feyerabend wants us to believe, has been discovered to arise 'together with theoretical assumptions not before them', and that 'an experience without theory is just as incomprehensible as is (allegedly) a theory without experience',<sup>1</sup> still, Feyerabend remarks, 'the inference that the distinction between theory and observation has now ceased to be relevant, is either not drawn or is explicitly rejected'.<sup>2</sup> Feyerabend does not deny that such a distinction can be made; he simply does not see its purpose. As he is of the opinion that the distinction between theory and observation has definitely lost its point,<sup>3</sup> he encourages us to take a step forward and, as he writes, 'abandon this last trace of dogmatism in science!'.<sup>4</sup>

However, in I. Hacking's view, all that Feyerabend has done is to show us 'how not to talk about observation, speech, theory, habits, or reporting'.<sup>5</sup> Hacking in fact dismisses the claim that observational reports always contain or assert theoretical assumptions as 'hardly worth debating because it is obviously false, unless one attaches a quite attenuated sense to the words, in which case the assertion is true but trivial'.<sup>6</sup>

On his part, Hacking puts forward a few examples from the history of science which he believes give substance to the view that observations, indeed important observations, have been made without any theoretical assumptions.<sup>7</sup>

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1. Feyerabend, 1979, p.168 (emphases in the original).

2. Ibid., p.169.

3. Ibid., p.168.

4. Ibid., p.169.

5. Hacking, 1983, p.175.

6. Ibid., p.174.

7. Ibid., pp.176-179. Hacking discusses this issue in his analysis of observation. (Ibid., Ch.10 (Observation); see also pp.155-156.)

However, we should not be drawn into this controversy; suffice it to note that there is a very great difference between asserting that all observational reports are 'convention-laden' and asserting that all observational reports are 'theory-laden'. Following P. Alexander, we can claim that in contrast to the former, the latter can be denied, and that it is therefore possible to have observational reports which are neutral as between theories in general, and as between scientific theories in particular. All uses of language are necessarily 'convention-laden', but we can distinguish, within any set of conventions, between what is merely 'convention-laden' and what is also 'theory-laden', so that descriptions which fall into the former class are, so to speak, pure, and that the distinction between description and interpretation, in at least one clear sense of those words, can be retained.<sup>1</sup>

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1. Alexander, 1963, pp.88-89.

## CHAPTER V

### Case-studies of Experiments

#### 5.1 R.A. Millikan's and F. Ehrenhaft's measurements of the charge of the electron

'A drop... is selected for observation, and its speed down under gravity, then its speed up under the influence of the electrical field, are measured with an almost unlimited degree of accuracy, since the operation can be repeated an indefinite number of times...'<sup>1</sup>

'This device [the oil-drop apparatus] is simply an electrical balance in place of a mechanical one, and it will weigh accurately and easily to one ten-billionth of a milligram.'<sup>2</sup>

'He who has seen that experiment, and hundreds of investigators have observed it, has literally seen the electron.'<sup>3</sup>

'... the electron itself... is neither an uncertainty nor an hypothesis. It is a new experimental fact that this generation in which we live has, for the first time seen,... which anyone who wills may henceforth see.'<sup>4</sup>

R.A. Millikan

'In my small horizontal condenser it is possible to compare the weight of a single charged sphere with the electric force acting upon the sphere, and therefore to determine the charge of the individual sphere without any averaging at all. I did this half a year earlier than the well-known oil-drop experiments done with condensers twenty times larger. I always found, and am still finding, charges smaller than the electron-charge. This result does not depend upon any assumptions whatever as to atomicity of matter or electricity.'<sup>5</sup>

F. Ehrenhaft

'In spite of the improvements in the Millikan oil-drop apparatus... the experiment remains perhaps the most frustrating of all the exercises in the undergraduate laboratory.'<sup>6</sup>

H. Kruglak

A direct method of determining the electric charge on ions was pioneered by J.S. Townsend in 1897.<sup>7</sup> As R.A. Millikan - one of the principal

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1. Millikan, 1910,b, p.284.

5. Ehrenhaft, 1940, p.385.

2. Millikan, 1922, p.101.

6. Kruglak, 1972, p.769.

3. Millikan, 1965, p.58.

7. Townsend, 1897; 1898.

4. Ibid., p.59.

protagonists of this controversy - remarked, this method was 'of much novelty and of no little ingenuity'.<sup>1</sup> Indeed, it contained some of the essential elements of subsequent methods of determining  $e$  and, in particular, set forth the two basic theoretical assumptions which have inhered in both Millikan's and Ehrenhaft's experiments. In this method water is deposited on ions in a gas by a condensation process produced by a sudden expansion; the number of ions present in the gas is deduced from the rate of fall of the cloud thus created whose overall charge can be measured directly. Historically, this method had evolved from researches on the ionization of gases - a phenomenon which drew much attention after the discoveries of x-rays and radioactivity.<sup>2</sup>

Townsend assumed in the first place that the rate of fall of the water droplets - droplets which constituted the cloud he observed in an expansion chamber - is proportional to the force acting upon them and is independent of the electric charge which the droplets are supposed to carry; a general assumption which he presupposed in order to facilitate the establishment of the law of motion of the cloud.

Since the cloud falls through a medium, it experiences resistance and reaches a terminal velocity which Townsend assumed to be given by Stokes' law. If a droplet of water of radius  $a$  falls uniformly under the action of gravity through a medium of viscosity  $\eta$  with a terminal velocity  $v$ , then its law of motion is given by the formula  $F=mg$  where, according to Stokes' law,  $6\pi\eta av$  is the force  $F$ , and  $(4/3)\pi a^3$  the mass  $m$ . Thus,  $a=((9/2)\eta v/g)^{1/2}$ . The weight  $w$  of each droplet may therefore be calculated and, having measured the weight of the cloud  $W$  per cubic centimetre of the gas, the number of droplets  $n$  per cubic centimetre of the gas can be inferred, that is  $n=W/w$ . A measurement of the charge on the ions in the gas may be carried out with a sensitive electrometer and thus the charge

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1. Millikan, 1922, p.43.

2. Holton, 1978, p.310 note no.62.

on one ion can be found, assuming of course that each droplet was formed on a single charge and that all of the charges were of the same kind.<sup>1</sup>

As we shall see, the general assumption which Townsend had presupposed was found later to be valid on independent experimental grounds, but the second assumption, namely, the application of Stokes' law in its original form, did suffer a setback in the process of its implementation in the various methods of determining  $e$ .

H.A. Wilson<sup>2</sup> simplified this method considerably by introducing in 1903 a charged condenser into the cloud chamber of the kind Townsend and J.J. Thomson<sup>3</sup> had used; he thereby eliminated some of the awkward assumptions they had made. By means of horizontal plates at the top and bottom of the expansion chamber, Wilson was able to observe the cloud as it moved subject not only to the gravitational force but also to an additional force, an electric force  $Eq$ , acting on each drop -  $E$  being the electric field strength between the plates, and  $q$  whatever charge was on a drop.

Wilson measured the rates of fall of the cloud when gravity operates alone, and when it is opposed by an electric force. Thus, for the former observation - assuming that the terminal velocities are proportional to the operative forces - one gets the equation  $v_g = Kmg$ ,  $K$  being a constant, and for the latter observation  $v_e = K(Eq - mg)$  is supposed to hold. Wilson obtained therefore the characteristic equation  $q = (mg/E)(v_e + v_g)/v_g$  without using Stokes' law. However, for the determination of  $q$ ,  $m$  has to be known, and as in Townsend's method Stokes' law is again referred to. Furthermore, whereas Wilson's reasoning refers to a single particle, he actually observed, as in the previous methods, the rates of fall of a cloud; he presupposed, therefore, that the droplets which comprise the cloud are identical - a

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1. Townsend, 1915, pp.238-39.

2. H.A. Wilson, 1903.

3. J.J. Thomson, 1898,a; 1899; 1903.

presupposition which is open to objections.<sup>1</sup>

The requirement to comply with the conditions of the experiment postulated in the theoretical treatment of the problem, motivated both Millikan and Ehrenhaft to search for an apparatus which would enable the experimenter to observe a single sphere. The sphere should be, ideally, infinitely removed from all other spheres and from the walls of any enclosure.<sup>2</sup>

Millikan and Ehrenhaft conceived their methods independently in 1909. Millikan observed the motion of single droplets of alcohol and water,<sup>3</sup> whereas Ehrenhaft, using an horizontal electric field and subsequently a vertical one, followed the motion of fragments of metal produced, for example, from the vapour of a silver arc.<sup>4</sup>

Operating his new technique Millikan could observe the 'history' of a single drop. He noticed that sometimes the upward velocity suddenly changed in a discontinuous manner from the value of  $v_e$  to another value, say,  $v_{e_1}$ , which may be greater or less than  $v_e$ . Millikan argued that this was due to the sudden change of the charge carried by the drop which apparently took place in a discontinuous fashion. He claimed that

by taking two speed measurements on the same drop, one before and one after it had caught an ion, I could obviously eliminate entirely the properties of the drop and of the medium and deal with a quantity which was proportional merely to the charge on the captured ion itself.<sup>5</sup>

This then was a method that could test directly the alleged atomistic feature of the electric charge even if Stokes' law were inapplicable to drops falling in a gas. For, if according to Wilson's equation

$q_1 = (mg/E)(v_{e_1} + v_g)/v_g$ , then after a sudden change from  $v_{e_1}$  to  $v_{e_2}$  the charge is  $q_2 = (mg/E)(v_{e_2} + v_g)/v_g$ . By dividing the one by the other one gets

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1. For an historical overview see Rayleigh, 1942, Ch.V (Cloudy Condensation and the Charge Carried by Electrons), and Ch.VI (More about Ions and Electrons).
  2. Perrin, 1920, p.172.
  3. Millikan, 1909.
  4. Ehrenhaft, 1909,a; 1909,b.
  5. Millikan, 1922, p.63.

$q_1/q_2 = (v_{e_1} + v_g)/(v_{e_2} + v_g)$ , or, to put it differently,  $q_1/(v_{e_1} + v_g) = q_2/(v_{e_2} + v_g) = q_3/(v_{e_3} + v_g) = \dots$ . Thus, if the sums  $(v_{e_1} + v_g), (v_{e_2} + v_g), (v_{e_3} + v_g)$ , etc., are found to be proportional to whole numbers, the successive charges born by the drop would be integer multiples of the same elementary charge.

It is important to note that the validity of this theoretical argumentation rests, among other things, on the constancy of the conditions of the experiment: the drop must retain the same mass;<sup>1</sup> the temperature should be kept constant throughout the experiment to prevent convection current and changes in the density and viscosity of the medium; the electric field should be ideally without any distortion and the condenser must be accurately placed in an horizontal position, and the like. Furthermore, as the measurements of the velocities are prone to errors arising from the operation of a stop-watch, they have to be allowed for. However, we are concerned here with the theoretical uncertainty as to the validity of the general assumption, namely, that 'the velocity with which the drop moves is proportional to the force acting upon it and is independent of the electrical charge which it carries.'<sup>2</sup>

Millikan subjected this general theoretical assumption to a careful experimental examination. He noted that the same drop can ascend under constant electric force with various velocities due, as he interpreted this phenomenon, to the drop's different charges. However, the drop always descends under gravity, within the limits of error of a stop-watch, with the same velocity no matter what charge it carries. Millikan concluded, therefore, that 'in these experiments the resistance which the

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1. Rutherford and Geiger pointed out that in these experiments 'it is assumed that there is no sensible evaporation of the drops during the time of observation of the rate of fall. There is no doubt, however, that evaporation does occur, and that the diameter of the drops steadily decreases'. (Rutherford and Geiger, 1908, p.170.) Cf., Millikan, 1910,a, pp.211-12, 218. To avoid excessive evaporation, Millikan started in 1911 to use non-volatile substances like oil and mercury. (Millikan, 1911, p.351.)

2. Millikan, 1922, p.68.



medium offers to the motion of a body... is not sensibly increased when the body becomes electrically charged'.<sup>1</sup>

The experimental confirmation of this theoretical consideration gave weight to the conclusion that when the electric charge on a certain drop in Millikan's apparatus changes, it always changes by  $ne$  -  $n$  being a positive or negative integer, and  $e$  some elementary charge. 'Here, then,' acclaimed Millikan, 'is direct, unimpeachable proof that the electron is not a "statistical mean", but that rather the electrical charges found on ions all have either exactly the same value or else small exact multiples of that value.'<sup>2</sup> However, in presenting his results, Millikan chose not to separate this demonstration from the determination of the magnitude of  $e$  - a closely connected problem but still a different one. In his papers he always combined Wilson's equation with Stokes' law, adding therefore another, more specific, theoretical assumption which requires in its turn a careful examination.<sup>3</sup>

Already in 1910, in his first major paper, 'A New Modification of the Cloud Method of Determining the Elementary Electrical Charge and the most Probable Value of that Charge',<sup>4</sup> Millikan indicated a certain awareness of the possible inapplicability of Stokes' law. He noted that in his method 'there is no theoretical uncertainty whatever left... unless it be an uncertainty as to whether or not Stokes' law applies to the rate of fall

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1. Ibid., p.84.

2. Ibid., p.70.

3. 'The problem of measuring the charge on the electron consisted of two closely related problems. The first was the question whether electric charge is quantized; the second, the accurate measurement of the value of the charge.' (Franklin, 1981,b, p.191.) In Perrin's view 'it is of greater advantage to put forward first of all the facts that would be unassailable even if Stokes' law were quite inapplicable to droplets falling in a gas'. (Perrin, 1920, p.174 footnote no.1.)

4. Millikan, 1910,a, pp.209-28.

of these drops under gravity'.<sup>1</sup> However, when he discussed, in the same paper, the results he had obtained, he rejected this cautious remark arguing that 'since this law has been shown by direct experiment to hold for large spheres falling slowly in a viscous fluid,<sup>2</sup> and since the spheres under observation in these experiments have diameters which vary from .00034cm. to .00047cm. - numbers which are from 35 to 50 times the mean free path of the air molecules - it is scarcely conceivable that Stokes' law fails to hold for them'.<sup>3</sup> Having assigned weights to his measurements, Millikan arrived at the final averaged result that the elementary charge is  $4.65 \times 10^{-10}$  esu.<sup>4</sup>

Millikan found corroboration for this result in Rutherford's and Geiger's measurement of the charge on an  $\alpha$  particle: they found in 1908 that an  $\alpha$  particle carries an electric charge of  $9.3 \times 10^{-10}$  esu.<sup>5</sup> Millikan however went on to suggest the possibility of reversing the argument of his experiment, claiming that 'if it be considered as proven that the  $\alpha$  particle is helium,... then the above observations may be taken as experimental verification of the validity of Stokes' law for water drops of the size used in this experiment'.<sup>6</sup> In other words, Millikan held that his result could constitute an experimental verification of Stokes' law for the drops he had used, for otherwise his experiment would become irreconcilable with Rutherford's and Geiger's experiment.<sup>7</sup> Thus, the theoretical assumption

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1. Ibid., p.219.

2. Millikan refers to the experiment of Allen, 1900.

3. Millikan, 1910,a, p.224 (my emphases).

4. Ibid., p.223.

5. Rutherford and Geiger, 1908, pp.168, 173.

6. Millikan, 1910,a, p.224.

7. Ibid., pp.224, 228. A student of Millikan executed such an experiment with precisely this intention in mind. See Ishida, 1923. Ishida writes in the abstract of his paper that 'now that  $e$  is known, the method may also be used to determine coefficients of viscosity'.

of the applicability of Stokes' law which Millikan had presupposed at the outset of his experiment, was purported to be verified at the end of the experiment by the very result whose background theory this assumption underlay.

It is of interest to note that with regard to his own experiment Millikan was not unambiguous, at least in his 1910 paper, of the ways Stokes' law functions logically and experimentally; he was however apprehensive of the pitfalls in Ehrenhaft's employment of this law. Indeed, he rejected Ehrenhaft's result principally on the basis of this issue - the usage of Stokes' law. In 1909, Ehrenhaft obtained  $4.6 \times 10^{-10}$  esu for the mean value of the electric charge by a method similar to that of Millikan, with the exception that it involved observations of metal particles collected from a metallic arc. This value conformed to Millikan's result; indeed, had it been accepted, it would have worked to Millikan's advantage in the sense that it would have brought the average value of the accepted determinations of  $e$ ,  $4.69 \times 10^{-10}$  esu,<sup>1</sup> down towards Millikan's result. Millikan however rejected it; as Holton remarks, Millikan rejected 'a confirmatory value, one obtained by an established researcher who had used a method closer to his own than the methods of others whom Millikan was not rejecting'.<sup>2</sup>

One of Millikan's principal objections to Ehrenhaft's result was based on the possible 'uncertainty as to the correctness of the assumption that Stokes' law is applicable to the motion of particles whose diameters are not negligible in comparison with the mean free path of gas molecules, and which are perhaps also of doubtful sphericity'.<sup>3</sup> To ensure the correctness of the application of this law, Ehrenhaft supplemented his observations of the rates of fall of his metallic particles with direct microscopic measurements of their radii; Ehrenhaft could thus confirm the computed values based on Stokes' law.<sup>4</sup> However, as Millikan pointed out, these

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1. Millikan, *ibid.*, p.227.

2. Holton, 1978, p.52.

3. Millikan, 1910,a, p.226.

4. Ehrenhaft, 1909,b, p.329; Ehrenhaft, 1910, p.630.

observed radii had been found to vary by more than 50 per cent., and since the mean value was but about  $3 \times 10^{-5}$  cm., it was quite clear that, as Millikan writes, 'even a moderate degree of precision in this measurement must be very difficult to obtain'.<sup>1</sup> Ehrenhaft's result, Millikan appeared to argue, was invalidated by the very method he had employed to obtain it; a plausible argument which Millikan, at that time, could not substantiate. Millikan, it seems, put forward this objection in an attempt to defend his own method and to demonstrate his awareness of the possible limitation of Stokes' law. In retrospect, it was a correct and shrewd move since it has dissociated Millikan's work from that of Ehrenhaft's right from the time of their inception in 1909 when the two methods produced similar results.

Following up his 1909 determination of the elementary charge, Ehrenhaft sought to increase the sensitivity of his apparatus and to achieve greater accuracy in these measurements by observing ultra-microscopic metal particles whose radii were of the order of  $10^{-5}$  cm. Assuming that Stokes' law for the resistance to the motion of a sphere in a viscous fluid holds for these particles, Ehrenhaft found that the particles investigated carried electric charges smaller than  $1 \times 10^{-10}$  esu.<sup>2</sup> Reporting this result in 1910, the journal Nature stated that this conclusion 'does not accord with the view so generally held at present that the "atom" of electricity is  $4.6 \times 10^{-10}$  esu'.<sup>3</sup> Half a year later the journal reported the conflicting views of Millikan and Ehrenhaft, and stressed that 'there is a serious difference of opinion between the Viennese and other observers on the fundamental question of the existence of atomic charges of electricity'.<sup>4</sup>

From 1910 onwards, Ehrenhaft continually found fractions of the 'atom'

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1. Millikan, 1910,a, p.226.
  2. Ehrenhaft, 1910, pp.619-30.
  3. Nature, 84(Aug. 11, 1910), p.182.
  4. Nature, 85(Jan. 19, 1911), p.383.

of electricity which he considered a demonstration of the existence of what he called subelectrons.<sup>1</sup> He declined to accept Millikan's objections and saw no reason why he should abandon Stokes' law in its original form. Fifteen years later, he was still holding fast to his results, claiming that if Millikan's criticism were to be valid, that is,

if... the validity of the resistance law for small moving spheres of mercury and other material is to be doubted, then it cannot hold, either, for other substances, e.g. oil. The withdrawal of such a premiss would of course cut the ground away from the elementary quantum idea... as deduced from observations of electric charges on individual oil drops.<sup>2</sup>

However, by the autumn of 1910, Millikan was already aware of the failure of Stokes' law when applied under the conditions of his own experiment and, what is more, he realized that the correction term is a variable depending on the size of the drop and other considerations.<sup>3</sup> He could thus account for the slightly inconsistent results he had previously obtained as well as for the diverse results of Ehrenhaft. In October 1910, at a meeting of the American Electrochemical Society, Millikan confidently maintained that

Ehrenhaft doesn't know what the trouble is. He concludes either that there isn't an elementary charge, or that it is much smaller than the value here obtained [that is, Millikan's value], or else that Stokes' law is not true. These results show that the last alternative is the correct one.<sup>4</sup>

Thus, according to Millikan the diverging results of Ehrenhaft were in effect the consequence of the failure of Stokes' law.

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1. Holton, 1978, pp.57, 317 note no.110.

2. Ehrenhaft, 1925, p.639.

3. Millikan published in 1911 his experimental research on the limitation of Stokes' law and the correction needed for his oil-drop experiment. (Millikan, 1911, pp.349-97.) While Millikan was in the process of finding empirically the correction to Stokes' law, the work of E. Cunningham appeared which examines this very problem from the theoretical point of view. (Ibid., p.380; Cunningham, 1910.)

4. Millikan, 1910,b, p.287.

Throughout his careful observations in 1910, Millikan found that the values of the elementary charges which were computed on the basis of Stokes' law, came out differently for different drops. Unlike Ehrenhaft, he did not assert the existence of subelectrons but rather re-examined the theoretical as well as the experimental uncertainties involved in his method. He was led to this process of critical reconsideration not only because of his well-entrenched belief in atomicity,<sup>1</sup> but also because of the glaring consistency of the value of  $e$  he had obtained for each individual drop. Millikan explained the irregularity of  $e$  carried by drops showing similar velocity under gravity (i.e. similar size) by the dirt effect of dust particles.<sup>2</sup> He subsequently attempted to eliminate this effect by 'blowing the drops into air which was strictly dust free, but even then,' Millikan admitted, 'drops of different sizes, as determined by  $v_1$  [the velocity under gravity], always gave consistently different values of  $e_1$  [ $e_1$  being the apparent value of the electric charge, i.e.  $e_1 = e_n/n$ ]'.<sup>3</sup> Characteristically, Millikan did not take it as a refutation of atomicity, but rather as a demonstration that Stokes' law does not hold for these drops. After all, Stokes' law is deduced from hydrodynamical principles of continuous flow and absence of slip, and it is most probable that it ceases to hold for spheres whose size is comparable with the mean free path of the molecules of the gas. Millikan thus sought 'to show that Stokes' law for the motion of a small sphere through a resisting medium, breaks down as the diameter of the sphere becomes comparable with the mean free path of the molecules of the medium, and to determine the exact way in which it breaks down'.<sup>4</sup>

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1. Holton's case-study of this dispute between Millikan and Ehrenhaft centres on the role of presupposition in the method of experimentation. (Holton, 1978, pp.25-83.)
  2. Millikan, 1911, pp.366-67.
  3. Ibid., p.367.
  4. Ibid., p.351.

Seeking this objective, Millikan made wide-ranging observations of 'drops the velocities of which varied in the extreme cases 360 fold. These velocities lay between the limits .0013 cm. per sec. and .47 cm. per sec.'. <sup>1</sup> Millikan realized however that this examination was in itself prone to further experimental uncertainties: 'when the velocities are very small residual convection currents and Brownian movements introduce errors, and when they are very large the time determination becomes unreliable'. <sup>2</sup> Clearly, one had to choose those drops which satisfied the optimum conditions.

Millikan concentrated therefore upon those large drops for which Stokes' law should most nearly hold and whose velocities under gravity were not too large. He reported that 'the times of fall of such drops under gravity were taken with a chronograph with as great care as possible. Also, wherever it was possible the same drop was timed by both Mr. Fletcher and myself in order to eliminate the personal equation'. <sup>3</sup> Here one sees Millikan at his best: combining critical theoretical inquiry with practical experimental insight, always looking for errors which might vitiate the observation.

Five years later, after perfecting his technique, Millikan was able to state with confidence that having eliminated dust 'all of my particles yielded exactly the same value of  $e$  whatever their size'. <sup>4</sup> In his critical examination of the evidence for the existence of subelectrons, Millikan contrasted the 'beautiful consistency and duplicability' of his results with the 'irregularities and erratic behaviour' of the results Ehrenhaft and his pupils had obtained. <sup>5</sup> In Millikan's view, non-uniform particles or particles which have dust specks attached to them would produce the same kind of erratic results.

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1. Ibid., pp.369-70.

4. Millikan, 1916,a, p.611.

2. Ibid., p.382.

5. Ibid., p.615.

3. Ibid., p.370.

While admitting that it would not be in keeping with the method of science to make any dogmatic assertion about the existence or non-existence of subelectrons, Millikan felt assured that it can be asserted that 'there is not in Ehrenhaft's experiments a scrap of evidence for the existence of charges smaller than the electron'.<sup>1</sup> In the conclusion of his critical examination of Ehrenhaft's results, Millikan maintained that these results are 'easily explained on the assumption of incorrect assumptions as to the density and sphericity of his particles, but even if these assumptions are correct, yet his results have no bearing on the question of the existence of a subelectron'.<sup>2</sup> Millikan strongly advocated the view that Ehrenhaft's results 'mean simply that he has assumed an incorrect law of movement of his minute charged particles through a gas'.<sup>3</sup>

J. Perrin concurred with this judgement. In his book, Atoms, he points out that Stokes' law in its original form 'does not accurately express the frictional force acting upon a microscopic spherule moving in a gas'.<sup>4</sup> Perrin explains that 'the expression holds for liquids, but in that case the radius is very great in comparison with the mean free path of the fluid molecules; whereas in gases it is of the same order of magnitude. The frictional effect will in consequence be lessened'.<sup>5</sup> This consequence becomes apparent when one considers that if the mean free path of the molecules of the gas were to become very great, or if, which comes to the same thing, the gas were to disappear, there would be no friction at all, although Stokes' law indicates that the friction is independent of the pressure.<sup>6</sup>

In Perrin's view, Ehrenhaft compounded the problem by using dust particles obtained from sparks between metals; particles whose homogeneous nature as spheres was open to objections. 'I am inclined to think,' Perrin writes, 'that they are really irregular, spongy bodies having an entirely

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1. Ibid., p.623.

2. Ibid., p.625.

3. Ibid.

4. Perrin, 1920, p.175.

5. Ibid.

6. Ibid.



irregular and jagged surface; their frictional effect in gases will be very much greater than if they were spheres, and the application of Stokes' law to them has no meaning'.<sup>1</sup> Perrin found confirmation for this view in Ehrenhaft's own report that the dust metallic particles have no appreciable Brownian movement, although they are ultra-microscopic.

According to Perrin, this very observation of Ehrenhaft indicates an enormous frictional effect.<sup>2</sup> By contrast, the experiments of Millikan in which use is made of droplets which possess a massive, close-grained structure obtained by atomizing liquids, are free from such objections. Perrin's conclusion is that 'Millikan's remarkable observations demonstrate in a vigorous and direct manner the atomic structure assumed for electricity'.<sup>3</sup>

When in 1925 O.J. Lodge - the editor of the influential journal Philosophical Magazine - agreed to publish Ehrenhaft's contention about the fundamental unit of electric charge as not an indivisible unit, he felt obliged to append the paper with Millikan's view of Ehrenhaft's experimental results. Somewhat apologetically Lodge spelled out the reason for publishing Ehrenhaft's paper, arguing that notwithstanding the general acceptance of the non-existence of subelectrons, 'the fundamental importance of the atomic nature of electricity, and the size of its ultimate unit, is so great that no serious attack on the orthodox position can be ignored'.<sup>4</sup> Fifteen years later, Ehrenhaft's experimental work on the fundamental unit of electric charge still received published editorial response. Commenting on his paper 'Physical and Astronomical Information concerning Particles of the Order of Magnitude of the Wave-length of Light', the editors of

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1. Ibid., p.172.

2. Ibid. Millikan, on his part, noted that he could not obtain consistency for oil droplets with radii less than  $3 \times 10^{-5}$  cm., since, as he explained, 'the displacements of such particles due to their Brownian movements became comparable with the displacements produced by gravity'. (Millikan, 1911, pp.393-94.)

3. Perrin, *ibid.*, p.173.

4. Lodge, 1925. Cf., Holton, 1978, pp.79, 321 note no.128.

the Journal of the Franklin Institute wrote that

while it is recognized that Prof. Ehrenhaft's conclusion as to the significance of his experiments are highly controversial, the experimental results themselves are such as to have recently excited the interest of several prominent authorities. The present paper is published with a full recognition of these factors and without comment for or against the views expressed.<sup>1</sup>

However, interest in Ehrenhaft's results gradually ceased and although he continued to publish them the controversy faded into obscurity.<sup>2</sup>

Ehrenhaft persisted in defending his experimental results not only because there was no direct experimental refutation of his own work, but also because he thought he had established decisive objections to Millikan's work. These objections can be generally characterized as logical and experimental: the logical objections amount to the claim that throughout his work Millikan presupposed atomicity and invariably begged the question; as to the experimental technique of Millikan, Ehrenhaft argued that it had not been sufficiently sensitive to detect fractions of what was considered the fundamental unit of charge.<sup>3</sup>

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1. Jour. Frank. Inst., 230(1940), p.381.

2. It appears that editors of recognized physics journals were reluctant to publish the work of Ehrenhaft and he was forced to seek publication elsewhere. (E.g., Ehrenhaft, 1941.)

3. Further arguments were adduced from the history of physics and the theory of electron or rather the lack of it. Examining in 1925 the historical trend, Ehrenhaft remarked that 'during the last three decades a series of phenomena have been observed in Physics which lead ultimately to the decision that the indivisible atom has to be broken up into smaller parts. What was considered indivisible up to a certain point, in time falls a victim at last to division. The case seems quite the same with the electron.... It is especially not clear why observations on individual charges should not show smaller charges, nor why the separate electrons cannot also be split up in the midst of such mighty energy changes as accompany radioactivity. And this is all the more true, since, as far as the writer is aware, there is up till now no theory of any kind to account for the electron's cohesion'. (Ehrenhaft, 1925, p.646.) Pursuing the same theme in 1941, Ehrenhaft argued that 'there are no theoretical reasons why all charges should be multiples of an elementary charge and why preferred values should be the same in all cases. Thus quantum hypotheses in one part of physics do not imply universality of quantization in other parts of physics or the indivisibility of the quanta elsewhere'. (Ehrenhaft, 1941, p.448.)

Ehrenhaft did not deny the correctness of Millikan's observations; rather, as he wrote in 1925, 'the discrepancy appears to lie... in the interpretation of the experimental results'.<sup>1</sup> In Ehrenhaft's view, Millikan 'had never had any doubt concerning the existence of an elementary quantum of electricity',<sup>2</sup> and he sought therefore to demonstrate this belief rather than to test it. Millikan's curious early method of qualifying the data as 'best', 'very good', 'good' and 'fair',<sup>3</sup> and, moreover, the practice of discarding data which seemed not to confirm atomicity as expected,<sup>4</sup> strengthened Ehrenhaft in his belief that Millikan's argumentation constituted an instance of petitio principii.<sup>5</sup> Indeed, Ehrenhaft was of the opinion that even the experimental determination of the corrections to Stokes' law which Millikan claimed to have established, contained the presupposition of an indivisible electron. Ehrenhaft argued that 'no significance can therefore be attached to resistance laws derived in this manner from which in turn the electronic charge is deduced, its existence, of course, being

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1. Ehrenhaft, 1925, pp.647-48.

2. Ibid., p.635.

3. Millikan, 1910,a, p.220.

4. Millikan reported that he had discarded observations mostly because the empirical conditions were uncertain. (E.g., *ibid.*) On one occasion he discarded 'one uncertain and unduplicated observation apparently upon a singly charged drop, which gave a value of the charge on the drop some 30 per cent. lower than the final value of  $e$ '. (*Ibid.*) Most disturbing however is when Millikan concluded that results which did not fit the expected curve had arisen from mistaking dust particles for oil drops; a conclusion based solely on the assumption of atomicity. (Millikan, 1916,a, p.610.) Carrying out the Millikan oil-drop method, a student of Millikan also found it advisable to discard some observations: 'there were a few cases that had to be discarded, the results being for some unexpected reasons 10 to 50 per cent out'. (Ishida, 1923, p.561.) On the analysis of Millikan's results see Franklin, 1981,b, pp.185-201; Fairbank and Franklin, 1982. Concluding their re-analysis of Millikan's original data, Fairbank and Franklin write that these data give 'strong evidence for charge quantization and no convincing evidence for fractional residual charge, although two events (out of 61) are consistent with such an interpretation'. (Fairbank and Franklin, *ibid.*, p.397.)

5. Ehrenhaft, 1925, p.639; Ehrenhaft, 1941, pp.406, 442-43.

by such methods easily demonstrable'.<sup>1</sup> According to Ehrenhaft, in 1925, 'the purely mechanical and experimental determination of the correction to be applied to Stokes' law for the motion of a small sphere in a gas has... not as yet been realized'.<sup>2</sup> Ehrenhaft did not cease to accuse Millikan of begging the question. Some fifteen years later he repeated the same allegation claiming that in Millikan's work 'that which ought to be proved by the experiments is presupposed, namely the existence and size of a smallest charge'.<sup>3</sup>

The allegedly fallacious reasoning of Millikan was compounded in Ehrenhaft's opinion by what Ehrenhaft considered a crude experimental technique. As late as 1941, Ehrenhaft still stressed the point that Millikan's 'apparatus is not capable of optical observation of particles smaller than  $3 \times 10^{-5}$  cm, and it is just on such particles that charges smaller than the expected elementary quantum are found most frequently'.<sup>4</sup> Indefatigably he argued that 'we can most probably expect to find the smallest electric charge on particles of the smallest capacity'.<sup>5</sup> However, as the test bodies have to be large enough to be optically perceived and subjected separately to physical measurements, 'we are... directed a priori', Ehrenhaft reasoned, 'to particles whose dimensions correspond with the limiting powers of the microscope'.<sup>6</sup> And that was indeed the original experimental problem Ehrenhaft had set himself in 1909: to measure 'the electric charge on the smallest possible individual particles and thereby subjected the

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1. Ehrenhaft, 1925, pp.639-40.

2. Ibid., p.640.

3. Ehrenhaft, 1941, p.442. Treating the experimental results statistically was according to Ehrenhaft another instance of circular reasoning. He insisted that his methods dealt only with individual particles and not with statistical sets. (Ibid., p.448. Cf., Ehrenhaft, 1925, pp.633-36.)

4. Ibid., 1941, p. 442.

5. Ehrenhaft, 1925, p.636.

6. Ibid.

foundations of the electric theory to a sharpest test'.<sup>1</sup>

In contrast to Millikan who concentrated on drops with radii which varied from  $2.5 \times 10^{-5}$  cm. to  $23 \times 10^{-5}$  cm., Ehrenhaft focused his research on particles whose radii varied between  $0.8 \times 10^{-5}$  cm. to  $2.5 \times 10^{-5}$  cm., but also extended his observations to particles of radii up to  $37 \times 10^{-5}$  cm.<sup>2</sup> On the basis of his experiments, Ehrenhaft reached the general conclusion that

test bodies below a radius of  $3 \times 10^{-5}$  cm. often carry charges smaller than that of the electron and, hence, that the charge on test bodies decreases on the average with their capacity (and hence radius). In this connexion charges of the order of a tenth or twentieth part of the electron's are by no means the smallest.<sup>3</sup>

Although Ehrenhaft did take cognizance of criticism directed at his experiments and improved his method by introducing independent tests of the sphericity, density and dimension of the particles investigated,<sup>4</sup> he never changed the essential features of his experiment. Instead of simplifying the set-up with a view to obtaining optimum conditions, he made the experiment more and more complicated and observed the particles under extreme conditions.<sup>5</sup> He indeed conceded that too many operations were required to complete the experiment, so much so that only seldom was it possible to complete it: 'the particle can get lost while it is being measured under different pressures, while being deposited on the small plate of quartz, while being immersed for the microscopic measurement, and on numerous other occasions'.<sup>6</sup> As Holton suggests, an 'ironic possibility for explaining

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1. Ibid., p.641.

2. Millikan, 1916,a, pp.617-18; Ehrenhaft, 1941, p.425.

3. Ehrenhaft, 1925, p.638. Cf., Ehrenhaft, 1941, pp.403, 447.

4. Ehrenhaft, 1925, pp.636-39; Ehrenhaft, 1941, pp.429ff.

5. Ehrenhaft's search for magnetic monopoles is another example of this attitude of seeking refuge, so to speak, in sophistication and complication: his experimental results could not be reproduced. (Benedikt and Leng, 1947; Kragh, 1981, pp.152-54.)

6. Ehrenhaft, 1941, p.434.

Ehrenhaft's results is that the equipment in Vienna was rather more sophisticated than necessary'.<sup>1</sup>

In comparison with the apparatus of Ehrenhaft, that of Millikan was indeed rather crude, but that was precisely an aspect of Millikan's strength: his observations were not taken under extreme conditions where one cannot be sure of the processes taking place, but rather under optimum conditions where the interference of other phenomena is minimal. As Millikan pointed out, Ehrenhaft had made his observations with an ultra-microscope and determined the rates of fall and rise of the particles investigated through exceedingly minute distances: about .01cm., in contrast to the 1.3cm. employed in the oil-drop method.<sup>2</sup> Millikan in fact attributed the accuracy and consistency of his results to the largeness of the distance of fall and the smallness of the magnification.<sup>3</sup>

According to Dirac, Ehrenhaft 'was certainly not a good physicist or he would have realized that there was very strong evidence for systematic errors in his work'.<sup>4</sup> Dirac did not specify what were in his view the sources of error in the measurements Ehrenhaft had carried out. However, from the perspective of the classification here presented, we conclude that the application of Stokes' law in its original form to the motion of the particles Ehrenhaft investigated, constitutes an instance of an error arising in the background theory: an error of the first kind.

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1. Holton, 1978, p.74.

2. Millikan, 1911, pp.392-93.

3. Ibid., p.393 footnote no.1. Cf., Millikan, 1965, pp.57-58.

4. Dirac, 1977, p.292. Cf., Holton, 1978, p.304 note no.12.

## 5.2 H. Hertz's experiment on the deflection of cathode rays in an electric field

'The electrostatic and electromagnetic properties of the cathode rays are either nil or very feeble.'<sup>1</sup>

H. Hertz

'An unfortunate experiment.'<sup>2</sup>

G.P. Thomson

(referring to Hertz's experiment on the nature of cathode rays)

In the second half of the nineteenth century an important and fruitful debate had taken place which was dissolved by the end of the century with the discovery of the electron by J.J. Thomson (1856-1940).<sup>3</sup> The debate focused on the physical nature of cathode rays: their behaviour and composition. By 1880 there was a general agreement that: 1. the rays are emitted from the cathode in an evacuated tube through which an electric current is passing; 2. the rays travel in straight lines; 3. the rays cause glass to fluoresce; 4. the rays are deflected by a magnetic field; 5. the rays are emitted perpendicularly to the surface of any cathode; 6. the properties of the rays are independent of the nature of the cathode material; 7. the rays can produce chemical reactions; 8. the rays could heat a thin foil to a red heat if focused on the foil; 9. the rays appear to exert a force, that is, they can convey momentum.<sup>4</sup>

Although the scientific community, particularly German and English physicists, agreed about these facts, there was a disagreement as to the composition of the rays; indeed, two diametrical views were put forward. The English school - including W. Crookes (1832-1919) and J.J. Thomson - held that the rays consisted of negatively charged molecules (ions), or

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1. Hertz, 1896, p.254 (emphasis in the original).

2. G.P. Thomson, 1970, p.237.

3. See, e.g., Owen, 1955.

4. Anderson, 1964, pp.23-25. Cf., H. Hertz, 1883, pp.798-99; an English translation of this paper can be found in Hertz, 1896, p.238.

negative corpuscles, while the Germans - including E. Wiedemann, E. Goldstein (1850-1930), H. Hertz (1857-1894) and P. Lenard (1862-1947) - were convinced that the cathode rays were some form of electromagnetic radiation similar to light.<sup>1</sup> The German physicists argued that the magnetic fields might deform the medium - that is, the medium through which the waves were thought to be moving - in such a way so it would deflect cathode rays but not ordinary light rays.<sup>2</sup> Likewise, the perpendicular emission of cathode rays could be attributed to their origin by electrical means, as opposed to the production of ordinary light by the mere heating of a surface.<sup>3</sup>

In the introduction to his conclusive paper of 1897, 'Cathode Rays',<sup>4</sup> J.J. Thomson set the scene of the debate as follows:

The experiments discussed in this paper were undertaken in the hope of gaining some information as to the nature of the Cathode Rays. The most diverse opinions are held as to these rays; according to the almost unanimous opinion of German physicists they are due to some process in the aether to which - inasmuch as in a uniform magnetic field their course is circular and not rectilinear - no phenomenon hitherto observed is analogous: another view of these rays is that, so far from being wholly aetherial, they are in fact wholly material, and that they mark the paths of particles of matter charged with negative electricity. It would seem at first sight that it ought not to be difficult to discriminate between views so different, yet experience shows that this is not the case, as amongst the physicists who have most deeply studied the subject can be found supporters of either theory.<sup>5</sup>

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1. For an historical overview see J.J. Thomson, 1898,b, 137ff.; on the theories of the nature of the cathode rays see in particular pp.189ff. Whittaker, 1916, pp.392-405; Rayleigh, 1942, Ch.IV (Cathode Rays and Corpuscles); G.P. Thomson, 1964, pp.30ff.
  2. 'Without attempting any explanation for the present, we may say that the magnet acts upon the medium, and that in the magnetised medium the cathoderays are not propagated in the same way as in the unmagnetised medium.' (Hertz, 1896, p.246.) The view that cathode discharge consists of 'an ether-disturbance, of itself invisible, and only converted into light by imparting its energy to the gas-particles', appeared to Hertz to be based, as he remarked, 'upon convincing arguments'. (Ibid.)
  3. Anderson, 1964, p.28.
  4. J.J. Thomson, 1897,b.
  5. Ibid., p.293. Thomson went on to argue for the corpuscular view on heuristic grounds: 'The electrified-particle theory has for purposes of research a great advantage over the aetherial theory, since it



The experiments J.J. Thomson reported in this important paper, were carried out with the intention of testing some of the consequences of what Thomson called the electrified-particle theory.

However, already in 1883 H. Hertz - the celebrated German experimenter and theoretician who had in 1877 demonstrated that oscillating currents do produce the electromagnetic waves predicted by Maxwell's equations - had set himself the task of reaching conclusive evidence with respect to the nature of cathode rays, precisely as J.J. Thomson did some fourteen years later.

Seeking an answer to the general problem: Have the cathode rays electrostatic properties? Herz formulated two auxiliary questions, namely, 'do the cathode rays give rise to electrostatic forces in their neighbourhood?' and, secondly, 'in their course are they affected by external electrostatic forces?'<sup>1</sup> To answer the first question, Hertz designed a cathode-ray tube whose cathode he surrounded with an anode of wire-gauze through which the cathode rays escaped to the far end of the tube, where they produced a fluorescent spot on the glass. Hertz introduced the gauze and, in addition to that, an earthed protecting metallic case which completely surrounded the greater part of the tube, so that all the space except that between the cathode and anode would be field-free. He enclosed that part of the glass tube which lay within the protecting case, in a metallic mantle which he connected to a delicate electrometer. He then calibrated the electrometer by introducing a quantity of electricity, similar to what he intended to charge the cathode, into the space of the metallic mantle, and recorded the electrometer's reaction. Hertz's argument was that 'if the cathode

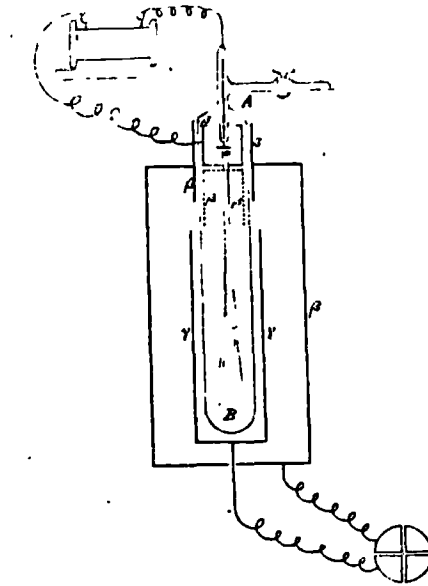
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footnote 5 from p.211 cont.....

is definite and its consequences can be predicted; with the aetherial theory it is impossible to predict what will happen under any given circumstances, as on this theory we are dealing with hitherto unobserved phenomena in the aether, of whose laws we are ignorant'. (Ibid., pp.293-94.)

1. Hertz, 1896, p.249.

rays consisted of a stream of particles charged to the potential of the cathode, they would produce effects quantitatively similar to the above [that is, the afore-mentioned calibration], or qualitatively similar if they produced any electrostatic forces whatever in their neighbourhood'.<sup>1</sup>



AB is the glass tube, 25mm. wide and 250mm. long, in which the rays were produced.  $\alpha$  is the cathode. All the parts marked  $\beta$  are in good metallic connection with each other, and those which lie inside the tube form the anode. The anode consists of a brass tube which nearly surrounds the cathode, leaving only opposite it a circular opening 10mm. in diameter, through which the cathode rays can pass. The rays have then to pass the second part of the anode which consists of wire-gauze, about 1 sq. mm. in mesh.

The anode is connected to an earthed protecting metallic case which completely surrounds the greater part of the tube and screens that part of the tube which lies beyond the wire-gauze from any electrostatic forces. At low pressures (Hertz did not specify them) the cathode rays cause the glass at B to shine with a brilliant green phosphorescence.  $\gamma$  is a metallic

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1. Ibid., p.250.

mantle which encloses that part of the glass tube which is placed within the protecting case;  $\gamma$  is connected to a delicate electrometer, whose needle deflected even when a small quantity of electricity was brought inside the metallic mantle  $\gamma$ . The electricity was introduced into  $\gamma$  by replacing the tube AB with a metal rod similar in size and position to the cathode-ray tube. The rod was connected to the cathode and charged by the induction coil in the same manner as the cathode was charged to produce the cathode rays. The recorded deflection of the electrometer's needle served as a reference to what would have been observed, according to Hertz, had the cathode rays consisted of a stream of particles charged to the potential of the cathode.<sup>1</sup>

Hertz ascribed to the corpuscular interpretation of cathode rays the view that the particles projected from the cathode should carry with them the charge of the cathode. Indeed, according to Crookes the constituent of the cathode rays were neutral molecules which picked up negative charges upon colliding with the cathode and were then repelled perpendicularly at high speed from the cathode's surface.<sup>2</sup> However, Hertz arranged his apparatus in such a way so that whatever the electrostatic forces produced in the neighbourhood of the cathode rays might be, they could have been detected by the delicate electrometer.

On trying the experiment he found the electrometer to respond irregularly, deflecting to not more than one-hundredth of what he observed in his preliminary calibration. Hence his conclusion that 'as far as the accuracy of the experiment allows, we can conclude with certainty that no electrostatic effect due to the cathode rays can be perceived; and that if they consist of streams of electrified particles, the potential on their outer surface is at most one-hundredth of that of the cathode'.<sup>3</sup>

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1. Ibid., pp.250-51.

2. Anderson, 1964, pp.26, 31. Cf. Conn and Turner, 1965, p.15; G.P. Thomson, 1964, p.32; Whittaker, 1916, pp.394-95.

3. Hertz, 1896, p.251.

Addressing himself to the same problem, Perrin got in 1895 definite evidence that the rays do carry a negative charge.<sup>1</sup> Two years later, J.J. Thomson modified Perrin's experiment and demonstrated that if the Faraday cylinder - that is, the metallic mantle - was put out of the line of fire of the cathode rays, it acquired a charge when, and only when, the cathode rays were so deflected by a magnet as to enter the cylinder.<sup>2</sup> It appears that this important work of Thomson was part of his grand design to refute the experimental results of Hertz; for like Hertz, he proceeded to examine the behaviour of cathode rays when subject to external electric forces.<sup>3</sup> But to return to Hertz's experiment.

Fairly convinced of this experimental finding that cathode rays do not have distinct electrostatic forces, Hertz turned to answer the second question that he had posed, namely, are cathode rays affected by external electrostatic forces? To this end, he placed a cathode-ray tube - similar to the one he had used in the preceding experiment - between two strongly and oppositely electrified plates, and looked for any deflection in the phosphorescent image. If the rays consisted of charged particles, then, subject to electric field, they should experience a force which would cause them to deflect. Hence, one would expect a deflection in the phosphorescent image at the far end of the tube. To obtain a distinct observation of such possible deflection, Hertz inserted a fine wire in the path of the cathode rays so that its sharp shadow on the phosphorescent patch at the end of the tube would serve as an accurate indicator of any deflection.<sup>4</sup>

Hertz reported that 'no effect could be observed in the phosphorescent image'.<sup>5</sup> He however realized that 'there was a doubt whether the large electrostatic force to which the tube was subjected might not be compensated

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1. Perrin, 1895.

4. Hertz, 1896, pp.251-2.

2. J.J. Thomson, 1897,b, pp.294-96.

5. Ibid., p.252.

3. Ibid., pp.296ff.

by an electrical distribution produced inside it'.<sup>1</sup> In other words, as G.P. Thomson explained, the surface of the glass might turn to be 'a good enough conductor to become charged by induction in such a way as to neutralize, or at least greatly reduce, the electric force between the plates.'<sup>2</sup>

Hertz inserted therefore the plates into the cathode-ray tube, connecting them to the battery by wires passing through the glass with airtight seals. In this experiment the tube's length was 26cm. and it was provided with a circular aluminium cathode 5mm. in diameter. The metallic plates, which were introduced into the tube to produce an electric field, were placed at a distance of 2cm. from one another. The rays had to travel a distance of 12 cm. between these plates.<sup>3</sup>

Hertz observed no deflection of the beam when using a battery of 20 small Daniell cells (approximately 22 volts) as a source of potential difference across the plates. Opening and closing the connection of the battery, he noted, 'produced not the slightest effect upon the phorescent image'.<sup>4</sup> He then charged the plates with 240 Planté cells (approximately 500 volts) which however discharged themselves through an arc-discharge upon setting in the cathode rays. Hertz, it appears, was actually expecting such a phenomenon, for he found it to be 'in accordance with Hittorf's discovery that very small electromotive forces can break through a space already filled with cathode rays.'<sup>5</sup>

Hertz's next step was to insert a large resistance between the battery and the plates; the battery discharged this time weakly and Hertz observed that 'the phosphorescent image of the Ruhmkorff discharge appeared somewhat

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1. Ibid.

2. G.P. Thomson, 1964, p.34.

3. Hertz, 1896, pp.251-2.

4. Ibid., p.252.

5. Ibid. Indeed, discharges through an arc occurred with a much smaller number of Planté cells: down to twenty or thirty. (Ibid.)

distorted through deflection in the neighbourhood of the negative strip; but the part of the shadow in the middle between the two strips was not visibly displaced'.<sup>1</sup> He thus concluded that 'under the conditions of the experiment the cathode rays were not deflected by any electromotive force existing in the space traversed by them, at any rate not by an electromotive force... [used here]'.<sup>2</sup>

Although Hertz admitted that this set of experiments was imperfect, he did hold that they enable us to decide that 'cathode rays cannot be recognized as possessing any electrostatic properties'.<sup>3</sup> And since he had shown by a different set of experiments that it is probable that cathode rays do not produce any strictly electromagnetic effects,<sup>4</sup> he felt justified in not regarding the cathode rays as an electrical phenomenon. It did not appear to him improbable that as far as the nature of cathode rays is concerned, 'they have no closer relation to electricity than has the light produced by an electric lamp'.<sup>5</sup> He finally concluded that 'the electrostatic and electromagnetic properties of the cathode rays are either nil or very feeble'.<sup>6</sup>

It is ironic that the prototype of the oscilloscope should be instrumental in demonstrating that 'cathode rays are electrically indifferent, and amongst known agents the phenomenon most nearly allied to them is light. The rotation of the plane of polarization of light is the nearest analogue to the bending of cathode rays by a magnet'.<sup>7</sup> Evidently, this conclusion

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1. Ibid.

2. Ibid., p.253.

3. Ibid.

4. Ibid., pp.238-49, 253.

5. Ibid., p.253.

6. Ibid., p.254 (emphasis in the original).

7. Ibid.

substantiated Hertz's conception of cathode rays as a wave phenomenon and refuted the corpuscular view of cathode rays as a stream of electrified particles.

Though the constituent of cathode rays, the electron, was found four decades later to be more elusive than a distinct corpuscle,<sup>1</sup> Hertz's conclusions were definitely in error. It is most probable that Hertz's failure to get a deflection was due to the accumulation of ions on or near the plates, which prevented the electric field between them being uniform and in effect protected the beam of cathode rays from really experiencing the lateral electric force which Hertz attempted to apply to them. In other words, owing to poor vacuum, the cathode rays ionized sufficient residual gas to permit neutralization of the plates; that in turn reduced substantially the electric-field intensity. It was, therefore, the conductivity produced in the residual gas that affected Hertz's experiment. It appears that it was this effect that prevented him from detecting the electrical charges known to be associated with cathode rays.<sup>2</sup>

The only experimental difficulty that Hertz recorded - apart from the difficulty that arose when the plates had been placed outside the cathode-ray tube - was concerned with the induction coil he had used. He noted that he had used the discharges of a small induction coil, and admitted that they were irregular and sudden, and indeed 'very ill adapted for electrostatic measurements'.<sup>3</sup> That, Hertz remarked, explained why the experimental results were not so sharp; however, as he maintained, 'the conclusion to which they lead may certainly be regarded as correct'.<sup>4</sup> Hertz did not

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1. On the emergence of the electron as a wave phenomenon see Elsasser, 1978, pp.59-64; G.P. Thomson, 1967.

2. Rayleigh, 1942, p.87; G.P. Thomson, 1964, pp.48-49.

3. Herz, 1896, p.249.

4. Ibid. G.P. Thomson remarked that Hertz was reluctant 'to press further with disproving what seemed the discredited fallacy that cathode rays were particles'. (G.P. Thomson, 1964, p.35.)

refer in any way either to the pressure inside the cathode-ray tube or to the evacuating technique he had used;<sup>1</sup> nor did he note that measurements of the electric potential across the plates had been taken. It seems very likely that had he measured it, he would have found that the voltage had been reduced substantially.

Since Hertz had been introduced to this field of study by Goldstein,<sup>2</sup> it is all the more surprising that he did not notice that his results contradicted one of Goldstein's experimental findings. In 1876, Goldstein observed a phenomenon which is now interpreted as an effect produced by the action of the electric force on the cathode rays. He used a cathode-ray tube which contained two cathodes situated side by side, and observed that when the two cathodes discharged simultaneously their rays repelled one another. Goldstein in fact used the German term abstossung (repulsion) to describe this phenomenon but, as<sup>he</sup> did not believe that cathode rays were particles, he considered the abstossung some special kind of force.<sup>3</sup>

This observation of Goldstein encouraged J.J. Thomson to repeat Hertz's experiment.<sup>4</sup> However, he at first got the same result, namely, no deflection. 'But,' as he remarked, 'subsequent experiments showed that the absence of deflexion is due to the conductivity conferred on the rarefied gas by the cathode rays'.<sup>5</sup> He measured this conductivity and found that it diminished very rapidly as the exhaustion increased. He suggested therefore

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1. At least not in this set of experiments; in other experiments on cathode discharge he recorded pressures ranging from one tenth to a few hundredth mm. Hg. (Hertz, 1896, pp.228, 241, 244-45.).
  2. 'I was first induced to undertake these experiments by conversations which I had with Dr. E. Goldstein as to the nature of the cathode discharge, which he had so frequently investigated. My best thanks are due to Dr. Goldstein for the ready way in which he placed at my disposal his knowledge of the subject and of its literature while I was carrying out the experiments.' (Ibid., p.224 footnote no.1.)
  3. Goldstein, 1876; G.P. Thomson, 1970, pp.239-40.
  4. J.J. Thomson, 1898,b, pp.169-79; J.J. Thomson, 1936, pp.333-34; G.P. Thomson, 1964, p.47.
  5. J.J. Thomson, 1897,b, p.296.



to try Hertz's experiment at very high exhaustions so that 'there might be a chance of detecting the deflexion of the cathode rays by an electrostatic force'.<sup>1</sup> And as he expected, it was only when the vacuum was sufficiently high that a deflection was observed.<sup>2</sup>

J.J. Thomson was in fact able to retrace Hertz's error and to clearly explain its occurrence. As he wrote:

...that the absence of deflexion is due to the conductivity of the medium is shown by what takes place when the vacuum has just arrived at the stage at which the deflexion begins. At this stage there is a deflexion of the rays when the plates are first connected with the terminals of the battery, but if this connexion is maintained the patch of phosphorescence gradually creeps back to its undeflected position. This is just what would happen if the space between the plates were a conductor, though a very bad one, for then the positive and negative ions between the plates would slowly diffuse, until the positive plate became coated with negative ions, the negative plate with positive ones; thus the electric intensity between the plates would vanish and the cathode rays be free from electrostatic force. Another illustration of this is afforded by what happens when the pressure is low enough to show the deflexion and a large difference of potential, say 200 volts, is established between the plates; under these circumstances there is a large deflexion of the cathode rays, but the medium under the large electromotive force breaks down every now and then and a bright discharge passes between the plates; when this occurs the phosphorescent patch produced by the cathode rays jumps back to its undeflected position.<sup>3</sup>

J.J. Thomson's papers on cathode rays virtually settled the debate; 'by explaining Hertz's error', G.P. Thomson commented, 'they removed the real objection to the particle theory of cathode rays.'<sup>4</sup>

G.P. Thomson in fact expressed a surprise that as great a physicist as Hertz could fall into such an error.<sup>5</sup> It is indeed surprising since Hertz had suspected the correct reason why the experiment failed when the plates were positioned outside the cathode-ray tube; and it is perhaps strange that he did not consider whether there might be a similar effect

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1. Ibid.

2. Ibid., p.297.

3. Ibid.

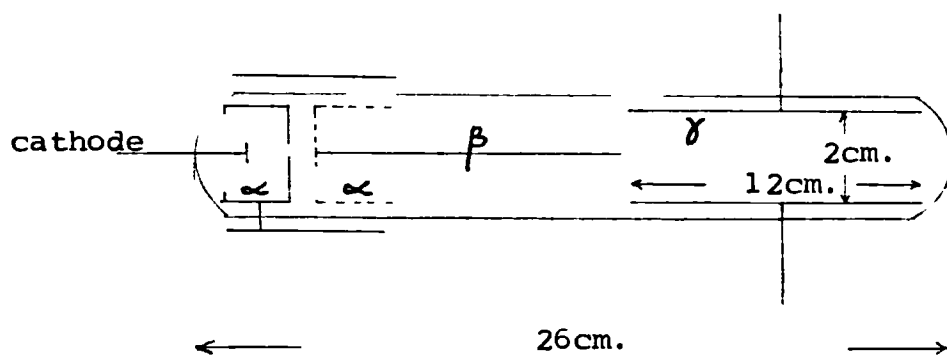
4. G.P. Thomson, 1964, p.51. Cf., Whittaker, 1916, p.405.

5. G.P. Thomson, 1970, p.237.

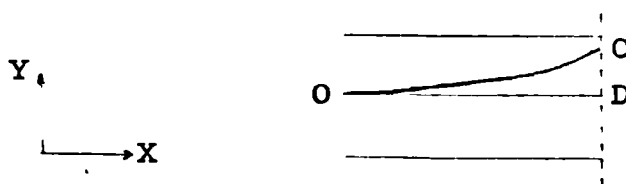
when the plates were placed inside the tube. Presumably, Hertz was convinced that he had proved that the rays did not exert electric force and it was almost impossible that they should be deflected by it. In G.P. Thomson's judgement, Hertz's results were by far 'the greatest objection to the particle theory of cathode rays';<sup>1</sup> and he regarded this work of Hertz on the nature of cathode rays as an unfortunate experiment.<sup>2</sup>

Given the geometrical arrangement of Hertz's apparatus, it is of interest to compute an upper limit for the electric potential across the plates which would have produced a distinct deflection had Hertz evacuated the cathode-ray tube sufficiently well.

The geometrical dimensions of Hertz's apparatus are as follows:



$\alpha$  - anode;  $\beta$  - a fine wire which casts a sharp shadow to serve as an accurate indicator of any deflection;  $\gamma$  - the condenser plates.



Let a charge moving with a velocity  $v$  along the X-axis parallel to the plates, enter the field at O. No force acts on the particle parallel to the X-axis, so its x-component of velocity remains unchanged, and the

1. Ibid., p.238.

2. Ibid., p.237.

distance travelled by the particle in time  $t$  is such that  $x=vt$ . The equation of motion for the  $y$ -coordinate is  $m d^2y/dt^2 = Ye$ ,  $Y$  being the intensity of the uniform electric field. Hence,  $my = (1/2)Yet^2$ , the constants of integration being zero, since  $y$  and  $dy/dt$  are both zero at  $t=0$ . Eliminating  $t$  from the equations, one arrives at the equation of the path, which is parabolic between 0 and  $C$ ,

$$y = Y(e/m)(1/2)(x/v)^2.$$

Now, between two parallel condenser plates charged to a potential difference  $\Phi$ , the uniform electric field has intensity  $Y = \Phi/d$ , if their distance apart is  $d$ , and  $d$  is very small compared to the dimensions of the plates. Hence, the equation for  $y$  is,

$$y = (\Phi/d)(e/m)(1/2)(x/v)^2.$$

In Hertz's arrangement,  $d=2\text{cm.}$ ,  $x=12\text{cm.}$ ,  $e/m=1.76 \times 10^7 \text{ emu. per gr.} = 5.28 \times 10^{17} \text{ esu. per gr.}$  (one emu. being equal to  $3 \times 10^{10} \text{ esu.}$ ), and, assuming the highest velocity for the charged particles,  $v=3 \times 10^9 \text{ cm. per sec.}$  A quick calculation shows that for a potential of 150 volts a deflection of about 1cm. should have been observed in Hertz's cathode-ray tube.<sup>1</sup> Since Hertz applied voltages that ranged from 22 to 500 volts, he could have obtained a distinct deflection, provided that his tube had been well evacuated. For comparison, in J.J. Thomson's apparatus a potential of 2 volts was sufficient for an observable deflection.<sup>2</sup>

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1. On the motion of a charged particle in an electric field see, e.g., Ruark and Urey, 1964, pp.22-24.
  2. J.J. Thomson, 1897, b, p.297. The reason is not only good exhaustion but also because of the geometrical arrangement of J.J. Thomson's apparatus which let the deflected beam fly in a straight line further on beyond the condenser plates. G.P. Thomson commented that 'since Hertz's work, fourteen years before [J.J. Thomson's work], pumps had improved, though they were still incredibly primitive by modern standards. This improvement was largely due to the demands of the electric lamp industry, one of the many cases in which industry had returned to pure science rich payment for the benefits which it has received'. (G.P. Thomson, 1964, p.49. On vacuum pumps see, *ibid.*, pp.175-81. Cf., Whittaker, 1916, p.392.)

Examining Hertz's error from the viewpoint of the classification I have presented, it can be seen that the second class, that is, the class of assumptions concerning the actual set-up and its working, subsumes this error. Hertz, in point of fact, was not aware of a certain physical phenomenon which prevented him from detecting the effect he was seeking. The conditions created in his apparatus, namely, poor vacuum, made it possible for the electric field to be neutralized - an effect which in turn eliminated the cause for the deflection of the cathode rays.

Consider, however, the question as to whether or not it is possible to detect a deflection - however feeble that may be - solely on the basis of Hertz's own records. If one were to answer this question in the affirmative then the error would belong to the fourth class: the theoretical conclusion would be incorrect. Now, this is precisely what G.F. FitzGerald (1851-1901) did when he reviewed in 1896 - on the occasion of the publication of Hertz's papers in English - these experiments of Hertz.<sup>1</sup> According to FitzGerald,

the experiments do not seem to fully justify the conclusion drawn, that kathode rays cannot be charged molecules. Sufficient account does not seem to have been taken of the shielding action of the conducting gas surrounding the kathode ray, nor of the way in which the potential is distributed between two electrodes in a gas.<sup>2</sup>

The crucial observational report in FitzGerald's view is that Hertz recorded

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1. FitzGerald writes of Hertz's papers that they 'are suggestive of questions which still require answers, and they all breathe a spirit that, as he says himself of Helmholtz's work, evokes "the same elevation and wonder as in beholding a pure work of art". His papers are not mere enumerations of observations, nor mathematical gymnastics. Each has a definite purpose and an artistic unity. A life-giving idea pervades it. It is no mere dry bones, but an organic whole that lives for a purpose, and does some work for science'. (FitzGerald, 1896, p.6.)
  2. Ibid., p.8. 'In the region of the cathode it was well known that there are strong electric fields; these would quickly drag away the ions as they were formed and the few left would be insufficient to neutralize the charges on the plates completely. Hence there was some deflection still detectable.' (G.P. Thomson, 1964, p.49.)

that 'the phosphorescent image of the Ruhmkorff discharge appeared somewhat distorted through deflection in the neighbourhood of the negative strip; but the part of the shadow in the middle between the two strips was not visibly displaced'.<sup>1</sup>

'This is exactly what one might expect,' FitzGerald commented, 'because the fall of potential between two such strips is very small indeed, except close to the negative strip, and there the electric force did deflect the rays. Hence,' in FitzGerald's view, 'the conclusion is just the reverse of the one Hertz gives. From the experiment,' FitzGerald concluded, 'it appears that cathode rays do behave like electrified particles'.<sup>2</sup> With this argument in mind, it can be maintained that Hertz's error is in fact of the fourth kind. Hertz misunderstood his correct observation: the phenomenon of deflection did show up - admittedly very feebly, yet sufficiently to be observed and recorded by Hertz himself - but he did not give it its due weight.

I suggest that these different characterizations of the one and the same error by the same system of classification do not contradict each other and, consequently, do not undermine the classification. I maintain that error is a phenomenon that becomes meaningful only when it is embedded within a set of ideas which claims to constitute knowledge. Thus, a classification of errors cannot claim a unique characterization since the characterization itself is made possible by the current knowledge of the phenomena.<sup>3</sup>

Today we are quite certain about what went wrong in Hertz's experiment. We have no doubts about the answer to Hertz's question: have the cathode rays electrostatic properties? The insight which J.J. Thomson gained from his experiments and further research in this field, enable us to answer

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1. Hertz, 1896, p.252; supra, p.217 footnote no. 1.

2. FitzGerald, op.cit.

3. I shall develop this theme in the last chapter.

this question firmly in the affirmative. FitzGerald, in contrast, had given his judgement on Hertz's experiments a year before J.J. Thomson published his conclusive work. Obviously, he was much influenced by the English view that cathode rays consist of negatively charged molecules, or negative corpuscles, and he quite rightly scrutinized Hertz's records in an attempt to turn the experimental argument against Hertz. Though he suspected a shielding effect, he thought that Hertz had erred in his theoretical conclusion. However, from our vantage point of view we conclude that Hertz erred in not realizing the consequence of a poor vacuum in such an experiment: an error of the second kind.

### 5.3 R. Blondlot's so-called discovery of N rays

'...the observer should accustom himself to look at the screen just as a painter, and in particular an "impressionist" painter, would look at landscape. To attain this requires some practice, and is not an easy task. Some people, in fact, never succeed.'<sup>1</sup>

R. Blondlot

'... it is time for French science to settle this question definitely. It must not become an object for the foreigner's irony. And we must not fear lest too superficial or too narrow minds should once again conclude from it that science is fallible! Science has demonstrated its power sufficiently to be afraid of nothing. It will give, on the contrary, a proof of its strength by recognizing madness; for it is the business of the scientific spirit not only to discover the true but to reveal the false. It has been said over and over that the N rays are the product of the imagination. Reason should control this assertion and make a final decision. This is why we have thought it proper to raise in France the question of the existence of these celebrated rays;... and no consideration, either personal or national, should, it seems to us, interfere with the sole object that we ought to have in view, namely, the truth.'<sup>2</sup>

#### Revue Scientifique

The Revue Scientifique polled leading scientists on their judgement of the existence of N rays.

Inquiry

Do the N rays exist?

The opinion of Prof. H. Moissan

Professor of chemistry at the Sorbonne and a member of the Institute

'Do you believe that scientific questions can be resolved by plebiscites?' Professor Moissan asks us.

'In any case, I have no opinion and I let the others do the experiments.' Yet, Prof. Moissan does not deny that he himself did some of the experiments; however, he does not reveal to what degree they have succeeded.

'Well,' he continues, 'I do have some ideas on that matter, but... well then I prefer not to have an opinion. Whatever I could say to further the issue would be less than a good experiment: let it be done!'<sup>3</sup>

H. Moissan

The discovery of X rays in 1895 posed - like the discovery of cathode rays - questions as to the nature of the phenomenon and its underlying constituents. The methods which generate these phenomena were well known

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1. Blondlot, 1905, pp.82-83.

2. Rev. Sci., 2 ser. 5(1904), p.552 (for the English translation see The Literary Digest, 29(Dec.17, 1904), p.840 ).

3. Moissan, 1904.

but their understanding was not immediately forthcoming. At the time when J.J. Thomson succeeded in solving the problem concerning the constituents of cathode rays, the nature of X rays remained a mystery and many efforts were directed at the resolution of this enigma. The principal question was of course were X rays particles or were they electromagnetic waves?

The standard practice to answer such a question was to make the rays pass through a charged condenser, and to observe whether or not a deflection took place. Another type of research was to attempt to induce polarization of the rays in question. A positive result in the former case would be interpreted as due to the motion of charged corpuscles, whereas the latter would indicate a wave phenomenon.

However, as René Blondlot (1849-1930) noted early in 1903, 'hitherto the attempts made to polarize "X" rays have remained fruitless'.<sup>1</sup> Blondlot, a professor of physics at the University of Nancy, was a well-known physicist. By 1903 he had published many papers in the respected journal Comptes Rendus, and was elected correspondent of the Academy of Sciences, Paris, to fill the vacancy caused by the death of Helmholtz. The Gaston Planté prize was awarded to him in 1893 for his studies on the propagation of electricity, and he received in 1899 the La Caze prize in physics. His association with Rayleigh, Hittorf, van der Waals, Michelson and H.A. Lorentz to form the group of Correspondents in General Physics of the Academy of Sciences at the beginning of 1905, suggests that his ability was highly respected.<sup>2</sup>

Since attempts to polarize X rays had failed, Blondlot asked himself 'whether "X" rays emitted by a focus tube are not polarized as soon as emitted'.<sup>3</sup> The conditions for asymmetry which should exist for polarization of such rays might be in this case satisfied; since, he reasoned, 'each

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1. Blondlot, 1905, p.1.

2. Stradling, 1907, p.179. For an extensive bibliography, see pp.189-99. See also, Rosmorduc, 1972.

3. Blondlot, op.cit.



ray is generated from a cathode ray, and the two rays define a plane; thus, through each ray emitted by the tube a plane passes, in which, or normally to which, the ray may well have special properties'.<sup>1</sup> Thus, in his February 1903 paper, 'On the Polarization of "X" rays',<sup>2</sup> - the paper which set the study of N rays in motion - Blondlot addressed himself to the problem of testing the suggestion that X rays are already polarized when emitted.

To this end, Blondlot constructed a detector which consisted of a pair of sharply pointed wires fixed opposite each other which were attached by loops to the two wires connecting the focus tube with the induction coil. Thus, whenever the coil discharged to produce cathode rays and, consequently, X rays, a spark would be produced between the sharply pointed ends of the wires. The small spark could play therefore, as Blondlot put it, 'the part of analyzer, in as much as the properties of a spark may be different in the direction of its length, which is also that of the electric force producing it, and in directions normal to its length'.<sup>3</sup>

Blondlot, it appears, expected himself to be able to distinguish visually between the brightness of the sparks which he intended to produce parallel and normal to the electric component of the supposed electromagnetic wave. Blondlot suggested that in the former case the energy of the spark would be reinforced and, consequently, its increased brightness would determine the polarized plane of the X rays.

Having defined the relative position of the tube and the spark, Blondlot observed that the spark did indeed increase its brightness, indicating by the relative position of its length the plane of the X ray and the cathode ray which gave rise to it.<sup>4</sup>

Blondlot stated that 'the phenomenon is easy to observe when the spark is well regulated; this means', he explained, 'that the spark must be very small and faint'.<sup>5</sup> As we shall see, this highly subjective method of

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1. Ibid.

2. Ibid.

3. Ibid., p.2.

4. Ibid., p.3.

5. Ibid., p.5.

observation which did not aim at objective criteria, constituted the major cause of this unhappy affair.

Although N rays are not mentioned in this paper, it did mark the beginning of the N-ray affair. Using this method of detection, Blondlot realized that he could detect radiation even when it suffered reflection and refraction;<sup>1</sup> he thus concluded that the rays which he had studied were not X rays, since, as he argued, 'these [that is, the X rays] undergo neither refraction nor reflection. In fact,' Blondlot continued, 'the little spark reveals a new species of radiations emitted by the focus tube, which traverse aluminium, black paper, wood, etc;'<sup>2</sup> hence the title of his paper of 23 March, 1903, 'Sur une nouvelle espèce de lumière'.<sup>3</sup> This new species of light was, according to Blondlot, plane polarized from the moment of its emission, susceptible of rotatory and elliptic polarization, refracted, reflected, diffused, but produced neither fluorescence nor photographic action.<sup>4</sup>

Summarizing the results of his work, Blondlot noted that at first he had attributed to X rays the polarization which, as he wrote,

in reality belongs to the new rays, a confusion which it was impossible to avoid before having observed the refraction, and it was only after making this observation that I could with certainty conclude that I was not dealing with Röntgen rays, but with a new species of light.<sup>5</sup>

Early in May, 1903, Blondlot communicated his further studies on the new rays which he claimed he had discovered. His studies were concerned with the transmission of this radiation through many substances and the search for its sources. Attempting a photographic method of observation, Blondlot reported that the rays produced no effect on a photographic plate when they acted for an hour. However, he produced two photographic plates which depicted the image of the spark once with the rays falling on it and

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1. Ibid., pp.8-11.

4. Ibid., p.11.

2. Ibid., p.11.

5. Ibid., p.12.

3. Ibid., p.7.

once without them: in the former case the spark was notably brighter.<sup>1</sup>

Later, in the same month, he reported that a small blue gas flame could be used to reveal the presence of these new rays which he designated with the letter N, after the name of Nancy University.<sup>2</sup> The flame could be used to reveal the presence of N rays just like the spark, since, according to Blondlot, 'when it receives these rays, it becomes whiter and brighter'.<sup>3</sup> Thus, the spark should be considered in this connection not an electric phenomenon, but rather a method for the production of incandescence, similar to the use of gas flame.

In the same paper Blondlot suggested a third method for the detection of N rays. It consisted of an already luminous calcium sulphide screen whose phosphorescent glow increased markedly upon being exposed to N rays. Although the production of this effect and its cessation were not instantaneous, Blondlot claimed that amongst all the actions produced by N rays, this was the most easily observed. And he was careful to stress that 'the experiment is an easy one to set up and to repeat'.<sup>4</sup>

Looking for more sources of N rays, Blondlot questioned the possibility that the sun may emit, in addition to known forms of radiation, N rays as well. He found out that this was indeed the case. 'The "N" rays issuing from the sun,' he observed on 15 June, 1903, 'increase the glow of a small spark and a small flame in the same manner as those emitted by a Crookes' tube, by a flame, or by an incandescent body'.<sup>5</sup> In Blondlot's view, these phenomena were easy to observe, especially when a small flame was used; he however admitted that the operation with a small spark was difficult since the spark was, in his words, 'rarely very regular'.<sup>6</sup>

The various researches of Blondlot and his co-workers at Nancy established that there were in fact many sources of N rays: artificial and

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1. Ibid., pp.16-17.

2. Ibid., p.20.

3. Ibid., p.21.

4. Ibid., pp.22-23.

5. Ibid., p.26.

6. Ibid., p.27.

natural. They showed that electric-discharge tube, Welsbach mantle,<sup>1</sup> Nernst glower,<sup>2</sup> and heated pieces of silver and sheet iron emitted the allegedly discovered rays. In addition to the sun as a natural source of the new radiation, it was claimed that the human body also issues such rays.<sup>3</sup>

The properties of the rays themselves were quite remarkable. Almost all the materials that were transparent to the rays, were opaque to visible light. Wood, paper, thin sheets of iron, tin, silver and gold were found to efficiently transmit these rays. Moreover, Blondlot showed that the rays - as a wave phenomenon - could be focused and bent by transmitting them through aluminium lenses and prisms. In contrast, water and rock salt absorbed the rays and thus could act as screens.<sup>4</sup>

By the summer of 1903, the newly discovered radiation had raised sufficient interest to become a major issue and many attempts were made, in France and elsewhere, to reproduce it. However, with the exception of France, all the results were negative. In the Versammlung deutscher Naturforscher und Ärzte at Cassel in September, 1903, Kaufmann of Bonn, Donath of Berlin, Classen of Hamburg, with Rubens and Drude, all acknowledged failure in their attempts to reproduce Blondlot's results.<sup>5</sup>

Undeterred by the negative results of other experimenters, Blondlot continued his researches into the properties of the radiation he had claimed to have discovered. We need not here enter upon an extensive study of Blondlot's results of which some are indeed exciting. Rather, we shall

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1. A type of gas burner widely used for home lighting around the turn of the century.
  2. A lamp in which a thin rod of rare-earth oxides was heated to incandescence by an electric current.
  3. Klotz, 1980, p.123. Cf., Stradling, 1907, pp.61-62, 64-66; see in particular the work of A. Charpentier.
  4. Klotz, *ibid.* Cf., Stradling, *ibid.*, pp.59ff.
  5. Stradling, *ibid.*, p.60.

dwell upon the negative outlook, that is, the criticism of Blondlot's results and his own awareness of the shortcomings of the methods of detection he had devised.

In 2 November, 1903, Blondlot made the following general remark concerning the observation of N rays.

The aptitude for catching small variations in luminous intensity is very different in different persons; some see from the outset, and without any difficulty, the reinforcing action produced by 'N' rays on the brightness of a small luminous source; for others, these phenomena lie almost at the limit of what they are able to discern, and it is only after a certain amount of practice that they succeed in catching them easily, and in observing them with complete certainty. The smallness of the effects and the delicacy of their observation must not deter us from <sup>1</sup>a study which puts us in possession of radiations hitherto unknown.

Blondlot suggested that use should be made of a 200 watt N<sup>n</sup>st lamp; with it 'the phenomena', he noted, 'are marked enough to be, in my belief, easily visible to any one at the first trial'.<sup>2</sup>

The end of 1903, the year in which Blondlot 'discovered' the N rays, saw the beginning of a stream of published notes and papers which recorded failure to reproduce Blondlot's results, advancing suggestions for sources of error and affording explanations of the phenomena claimed to be observed.

O. Lummer, for example, maintained that

a whole set of Blondlot's experiments may be almost exactly imitated in their effects without employing any source of illumination whatever, and that the changes in form, brightness, and colour... observed by Blondlot under a stream of rays,... may be referred to processes taking place in the eye itself, and, in fact, to the contest between the rods and cones of the retina in seeing in the dark.<sup>3</sup>

Lummer warned physicists who were attempting to repeat Blondlot's experiments that 'in vision in the dark changes in brightness, form and colour may arise from a purely subjective source'.<sup>4</sup> These purely subjective changes

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1. Blondlot, op.cit., p.37.

2. Ibid., p.38.

3. Lummer, 1904, p.378 (emphases in the original).

4. Ibid., p.380.

were not, Lummer maintained, optical illusions but rather true phenomena which, however, were brought about by the structure of the visual organ and corresponded, therefore, to objective processes in the retina. Lummer concluded his criticism by calling for an incontestable proof of the existence of N rays by means of objective instruments of precision.<sup>1</sup>

A.A. Campbell Swinton, in a letter to the editor of Nature, made similar suggestions. He communicated in 21 January, 1904, that he had repeated most of Blondlot's experiments, but was unable to discern the slightest trace of any of the remarkable phenomena that Blondlot claimed to have observed. In order to eliminate personal physiological idiosyncrasies, he applied a delicate photographic technique, but did not obtain any positive result. He suggested therefore 'that Blondlot's observations must be due, not to physical, but to physiological processes, and further, that these are not operative in the case of all persons'.<sup>2</sup>

In an attempt to satisfy his critics, Blondlot contrived a new photographic method which relied upon the alleged fact that cardboard was transparent to N rays. Blondlot enclosed the detector in a cardboard box, in which a photographic plate was placed. When the N rays fell on the box they could pass through it and make the spark brighter, which in turn darkened the photographic plate. Then, a lead screen wrapped in wet paper which was believed to absorb N rays, was introduced between the N-ray source and the cardboard box, placing thereby a new photographic plate; the latter plate was expected to be brighter in comparison with the former since, other things being equal, the spark got dimmer. Using this new technique, Blondlot reported in February, 1904, that he had made 40 experiments which were witnessed by several eminent physicists. Only one experiment was unsuccessful: he got two sensibly indistinguishable images. Blondlot suggested that it might have occurred due to an insufficient regulation

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1. Ibid.

2. Swinton, 1904,a, p.272.

of the spark. However, the success of his photographic results made Blondlot confident that he had succeeded in recording the action of N rays on the spark by an objective method.<sup>1</sup>

Closely afterwards, Blondlot reported the discovery of a new type of radiation. While studying the feebly deviated part of the spectrum of the emission of Nernst lamp produced by an aluminium prism, Blondlot discovered that in certain azimuths the glow of the spark diminished under the action of the rays, and increased, contrary to expectation, when they were intercepted by a wet screen. He designated this new radiation, which acted in an opposite manner to the N rays, as  $N_I$  rays.<sup>2</sup>

This discovery provided Blondlot with an explanation for the fact that only the observer placed exactly in front of the sensitive screen could perceive the effects of N rays. He argued, therefore, that it would be illusory to try to make an audience witness these experiments since the effects perceived by different persons were dependent on their positions relative to the screen; the N-ray effects would be simply imperceptible to almost all of the audience in a hall.<sup>3</sup>

Fortified with his new results, Blondlot addressed a letter - in the face of mounting criticism - to the Electrician, London. In the correspondence column of this journal of 11 March, 1904, Blondlot related that Mascart, the president of the Academy of Sciences, and another member of the Academy, came to Nancy to see his experiments.

They saw and repeated them themselves, they determined themselves the deviations of the N rays by a prism of aluminium, and the focusing of them by an aluminium lens, they observed the fringes produced by gratings, Newton's rings, etc. They did not show the least doubt in the existence of the N rays. M. Mascart took away with him a double photograph, taken in his presence, or rather with his co-operation... A few days later I was visited by M.J. Becquerel<sup>4</sup> who observed without difficulty the N rays phenomena, and who proposed in parting to follow up the studying of some of them.

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1. Blondlot, 1905, pp.61-68.

3. Ibid., pp.72-73.

2. Ibid., pp.68-69.

4. The son of H. Becquerel.

Blondlot disclosed that Sir William Crookes invited him to deliver a lecture on N rays before the Royal Institution in London. 'It is certain,' Blondlot maintained, 'that he would not have made such a proposal had he had the slightest distrust even in the subject of my work.' Blondlot emphasized that the phenomena he had discovered were not the result of the actions of heat, and he added that he had not published one single experiment which had not been repeated by several of his colleagues and by a few laymen. He ended his letter by advising the physicists who were studying these phenomena,

to avoid every effort and tension of the eyes, every fixation. Otherwise psycho-physiological phenomena occur, of the kind observed by M. Lummer. Although the actions of the N rays require delicate methods to be observed (they would otherwise have been detected long ago) I believe that investigators will be compensated for the trouble expended, inasmuch as I have encountered but three or four persons <sup>1</sup> out of a great number - who were not able to observe the phenomena.

One may note at this juncture the clear formation of two camps: the followers of Blondlot and the critics. The followers of Blondlot - those who considered the existence of N rays a scientific fact - proceeded with great zeal to put this discovery into practice. An apparatus for N-ray experiments was put on the market, and the respected medical journal, the Lancet, afforded space for a discussion about the use this new form of radiation could be put to.<sup>2</sup> In contrast, the critics maintained their sceptical view and continued in their search for sources of error and other possible explanations. Swinton reported in the Lancet that

it is by no means difficult to get some of the effects that M. Blondlot... and your correspondents describe, but so far, my observations go, these effects when obtained are in every case due simply to heat and require of no new description of rays for their explanation. The luminosity of calcium sulphide, as also of most other phosphorescent substance, is considerably affected by minute difference of temperature.<sup>3</sup>

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1. Blondlot, 1904,a, p.830.

2. Stradling, 1907, p.69. Cf., Lancet, 1(1904).

3. Swinton, 1904,b, p.685.



Although Swinton maintained that N rays had no real objective existence, he was prepared to admit that, as he put it, 'there may be effects visible to some persons to which I myself and my assistants and others who have also obtained negative results are blind. It may, in fact, be a matter similar to "colour blindness" only in another form'.<sup>1</sup> And he concluded by warning experimenters in this 'most interesting field' that the accurate observation of the dim phosphorescent objects was very difficult indeed, since the different parts of the human retina are very unequally sensitive to faint lights, while optical fatigue and other phenomena might obscure the results.<sup>2</sup>

H. Walsham and L. Miller, the correspondents to whom Swinton referred, responded by admitting that they were not sufficiently careful to eliminate heat effects; they, however, pointed out that Swinton and others were at that time publicly stating that they could get no results at all, and were inclined to believe that the N-ray phenomena were subjective. So, either Swinton got the expected effects - no matter whether the cause was N radiation or simply heat - or he did not; he could not, Walsham and Miller seemed to argue, have in his criticism the cake and eat it. In their view, N rays behaved indeed like heat and to support the view that heat was not the cause of the effects, they recommended a perusal of Blondlot's letter in the Electrician. They however disclosed that they could not find out why their experiments occasionally failed; they suggested that their eyes, not the rays or the screen, were at fault. They therefore warned the readers against attempting any trials except in the early morning or after sunset, unless they prepared their vision by shutting themselves in a dark room for about half an hour before the trials took place.<sup>3</sup>

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1. Ibid.

2. Ibid., pp.685-86.

3. Walsham and Miller, 1904.

J.B. Burke, of the Cavendish Laboratory, suggested, like Swinton, that Blondlot had discovered a radiation to which some people are blind. In his view, the effects which Blondlot observed were, as he put it, 'in the true sense', objective and not subjective. He however qualified his remark by pointing out that the precise conditions upon which the effects depend remained to be discovered.<sup>1</sup>

C.C. Schenck, of McGill University, also reported that he had obtained positive results which he, however, could explain without resorting to a new form of radiation. He argued that the differences of brightness which he had observed might have been due to either decay of phosphorescence, or obliquity of angle of vision, or increase in the sensitivity of the eye in the dark, or finally, heat effects. Schenck also pointed out that some of Blondlot's results could not be obtained, on the ground of optical principles, no matter whether N rays exist or not.<sup>2</sup>

So far all the arguments which the critics directed against Blondlot's results, were based on either physical or physiological theories. Another mode of criticism emerged in April, 1904, when J.G. McKendrick and W. Colquhoun, of Glasgow University, reported uniformly negative results in experiments on what they called Blondlot's rays. 'How is it,' they asked, 'that he [Blondlot] and many of his compatriots see increase of brightness

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1. Burke, 1904,a, p.365.

2. Schenck, 1904, pp.486-87. According to Schenck, the alleged beams of different wavelengths of N rays which emerged from an aluminium prism in one of Blondlot's experiments, could not have been so well defined as Blondlot had claimed: some of them should have overlapped. Similarly, diffraction fringes were not to be believed in since they must have been lacking in definition. Finally, Schenck criticized the Newton's rings experiment claiming again lack of definition; he went on to remark that 'it would be interesting to know just where the phosphorescent screen was placed in this experiment, as the rings are formed in the thin air gap between the lenses, and the eye must be focused on that point to see them sharply. But of course, the screen could not be put between the lenses, as the latter could not then be brought into close contact, and if it were placed anywhere else the rings would be somewhat blurred'. (Ibid., p.487.)

under conditions in which we see none?'<sup>1</sup> They suggested that the explanation might be found in the paper, 'Die Aufmerksamkeit und die Funktion der Sinnesorgane', by Heinrich, in which he demonstrated that mental calculation can cause physiological changes in the eye, such as change of muscle tension. 'Can it be,' enquired McKendrick and Colquhoun, 'that the mental condition of some observers in a state of expectancy reacts on the... muscles of their eyes, and thus they see what they think they should see?'<sup>2</sup>

Although much scepticism was expressed with regard to the objective existence of the N-ray phenomena, the spring of 1904 saw a new crop of experimental results of N-ray effects; and, later on, in July, the stream of published papers reporting positive results reached its peak. Indeed, it appears that the proponents of the N-ray phenomena had never advanced with more confidence - on what they believed was a firm ground - than in that month. And yet the excitement ceased with a remarkable abruptness: not a single paper recording N-ray investigation appeared during August, September and October of that year;<sup>3</sup> it was then the turn of the critics to advance their arguments. Specifically, several editorial comments were made as to the validity of the experimental methods used, and stronger objections were put forward at the meetings of German and British associations, as well as at the International Congress of Physiology in Brussels.<sup>4</sup> From May of that year, when no criticism was published, objections gathered momentum which culminated in September with the visit by the American physicist, R.W. Wood, to Blondlot's laboratory at Nancy, where, as Wood put it, 'the apparently peculiar conditions necessary for the manifestation of this most elusive form of radiation appears to exist'.<sup>5</sup>

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1. McKendrick and Colquhoun, 1904, p.534.

2. Ibid.

3. Stradling, 1907, p.119.

4. Ibid., pp.119-21.

5. Wood, 1904, p.530. Cf., Stradling, *ibid.*, p.121.

Already in June, Burke asked for an explanation of the reported results, be they subjective or objective. 'Psycho-physiological phenomena are not the less interesting,' he maintained, 'because they happen not to be physical effects as ordinarily understood, and if they can lead scores of trained physicists astray, they should be regarded as all the more important.'<sup>1</sup> In Burke's view, expectation and concentration of attention were important requisites for a positive observation.

Following Blondlot, Burke attempted to detect the N-ray effects photographically. As he could not obtain positive results he argued that in Blondlot's photographic method there was no proof that the diminished brightness of the spark upon interposing a lead wet screen was not due to the presence of the metallic screen itself, which, being so close to the spark, damped its oscillations and consequently affected its photographic effect. Burke preferred to put out the source of N rays altogether, or to place the screen at a considerable distance from the spark; he reported that 'except when there were errors in the adjustment of the apparatus, the two photographs... indicated the same brightness'.<sup>2</sup>

As Wood noted, it was 'the inability of a large number of skilful experimental physicists to obtain any evidence whatever of the existence of the n rays, and the continued publication of papers announcing new and still more remarkable properties of the rays', that prompted him to pay Blondlot a visit.<sup>3</sup> At the outset of his letter to the editor of Nature, in which he related this visit, he stated that having spent three hours or more witnessing three different experiments, he was not able to report a single observation which appeared to indicate the existence of the rays; moreover, he left Nancy<sup>4</sup> with, in his words, 'a very firm conviction that

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1. Burke, 1904,b.

2. Ibid.

3. Wood, op.cit.

4. Though Wood did not mention names, they are implied.

the few experimenters who have obtained positive results have been in some way deluded'.<sup>1</sup>

In the first experiment which Wood was shown, N rays were focused, by means of an aluminium lens, upon a small electric spark whose brightness was expected, as a consequence, to increase. Wood was thus presented with an improved version of the original experiment which had allegedly revealed the new radiation. To observe change of luminosity, the observer had to interpose his hand between the spark and the source of N rays. The experimenters, Wood reported, had claimed that the change 'was most distinctly noticeable, yet I', he maintained, 'was unable to detect the slightest change'.<sup>2</sup> The negative result was explained to him as due to an insufficient sensitivity on the part of his eyes. Wood did not succumb and arranged to have another observer tell when he, Wood, interposed his own hand in the path of the rays. He instructed the observer to announce the exact moments at which he introduced his hand only by observing the screen upon which the light of the spark was projected. 'In no case,' Wood reported, 'was a correct answer given, the screen being announced as bright and dark in alternation when my hand was held motionless in the path of the rays, while the fluctuations observed when I moved my hand bore no relation whatever to its movements.'<sup>3</sup>

Wood considered the photographic plate which was exposed in his presence - a photograph purporting to record the change that occurred in the spark's brightness - an inadmissible evidence. He argued that it had been produced under conditions which admitted of many sources of error. Specifically, the brilliancy of the spark fluctuated in such a manner that it alone would render accurate work impossible. And, secondly, Wood suggested that since the experimenter had previous knowledge of the disposition of

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1. Wood, op.cit.

3. Ibid.

2. Ibid.

the apparatus with which the photograph was taken, a cumulative favouring of the exposure of one of the images might have taken place.<sup>1</sup>

The second experiment which Wood witnessed demonstrated the deviation of N rays by an aluminium prism; the prism, it had been claimed, not only could bend the rays but also spread them out into a spectrum. To locate the rays of different wavelengths, the experimenters used a screen which could move along by means of a dividing engine. It was claimed that a movement of the screw of the dividing engine corresponding to a motion of less than 0.1mm was sufficient for moving from one spectral line to another. When Wood, like Schenck, expressed surprise as to the possibility of splitting up a ray bundle 3mm. in width into a spectrum with maxima and minima less than 0.1mm. apart, he was told that 'this was one of the inexplicable and astounding properties of the rays'.<sup>2</sup> Needless to say that Wood could not detect any change of brightness as he turned the screw. He then removed covertly the prism - 'we were in a dark room',<sup>3</sup> he remarked - and, perhaps not to his surprise, the removal of the prism did not seem, as he wrote, 'to interfere in any way with the location of the maxima and minima in the deviated(!) ray bundle'.<sup>4</sup> He subsequently suggested to place the prism in different positions while the N ray expert would have to determine, using solely the screen, the position of the prism. Incorrect answers were given in all of the three attempts. 'The failure,' Wood reported, 'was attributed to fatigue.'<sup>5</sup>

The third experiment consisted of demonstrating the emission of N rays from a steel file, and their peculiar property to enhance vision when they fell on the retina. A special screen was employed for the first part of the experiment, but Wood could not notice any change in its brightness

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1. Ibid.

4. Ibid.

2. Ibid.

5. Ibid.

3. Ibid.

when the file was brought near the screen. The phenomenon, however, was described by the experts as 'open to no question, the change being very marked'.<sup>1</sup> In the second part of the experiment the room was dimly lit and Wood was asked to hold the file close to his eyes, thus allowing the N rays to impinge upon the retina, and to observe an increase in the acuteness of his eyesight. Wood did not sense the slightest change; the dimly lit objects did not appear more distinct or brighter. Though Wood could not perform under these circumstances control experiments - what he called 'blank experiments' - he did succeed in changing the file without the knowledge of the observer to a wooden one; however, the change did not affect the positive results obtained by the Nancy observer.<sup>2</sup>

Wood concluded,

I am obliged to confess that I left the laboratory with a distinct feeling of depression, not only having failed to see a single experiment of a convincing nature, but with the almost certain conviction that all the changes in the luminosity or distinctness of sparks and phosphorescent screens (which furnish the only evidence of n rays) are purely imaginary. It seems strange that after a year's work on the subject not a single experiment has been devised which can in any way convince a critical observer that the rays exist at all.<sup>3</sup>

Wood ended his letter by suggesting experiments which in his view could resolve the issue once and for all, and by calling for more collaboration between experimenters; 'why cannot', he asked, 'the experimenters who obtained results with n rays and those who do not try a series of experiments together?'<sup>4</sup>

In reply Blondlot argued that Wood had been warned that a person observing a phosphorescent screen could not necessarily tell when another person flashed the N rays on the screen. 'I affirm most positively,' he said, 'that the phenomena of N rays have for me the same certainty that

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1. Ibid.

2. Ibid., pp.530-31.

3. Ibid., p.531.

4. Ibid.

other physical phenomena have. Several of my colleagues and a number of other persons say the same.'<sup>1</sup>

To counter Wood's allegation that the photographic method was biased, Blondlot devised a new method which was automatic and slightly biased to the benefit of the plate which was used when the N rays were shut off. The developed plates still confirmed Blondlot's claims. Moreover, responding to Burke's criticism, Blondlot demonstrated that the lead screen did not interfere with the spark when the N-ray source was turned off.

In October, 1904, the Revue Scientifique began an editorial discussion on the status of the N rays, inviting opinions on the subject from many French scientists. Among the critics were Langevin and Perrin; the latter was very determined in his remark. 'I think and repeat on every occasion for more than a year,' Perrin stated, 'not only that there are no N rays, but that there are no objective phenomena capable of justifying the strange error of those physicists who have seen the rays.'<sup>2</sup> Yet among the proponents were some celebrated scientists: Berthelot, H. Poincaré, H. Becquerel, Bichat and, of course, Blondlot.<sup>3</sup>

It is worth noting that some of those who believed in the disputed rays could not get positive results when they themselves experimented with them. Poincaré, for example, visited the N-ray laboratory at Nancy but saw no effects. He attributed his failure to involuntary accommodation of the eyes on his part. Furthermore, he considered the photographic effect of the N rays real.<sup>4</sup>

Thus, in spite of the fact that by the end of 1904 the critics succeeded

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1. Blondlot, 1904,b, p.621. Quoted by Stradling, 1907, p.121.

2. Perrin, 1904, p.623. Quoted by Stradling, *ibid.*, p.122 (emphases in the original).

3. *Rev. Sci.*, 2 ser.5(1904), pp.73-79, 545-52, 590-91, 620-25, 656-60, 682-86, 705-9, 718-22, 752-55, 783-85.

4. Poincaré, 1904, p.682. Cf., Stradling, 1907, p.123. See also the opinion of Pellat. (*Rev. Sci.*, 2 ser. 5(1904), pp.590-91.)



in consolidating their position as to the non-existence of N rays, positive experimental results still abounded. The Lancet, for example, again published such results; this time from J. Stenson Hooker who opened his letter by claiming that 'there are few now who do not acknowledge the existence of the "n" rays'.<sup>1</sup> Hooker adduced some historical evidence to the effect that the concept of colour was slow to get recognition; in Sanskrit, for example, there is no word for colour, and Xenophanes knew of only three colours of the rainbow. 'We can easily imagine the wiseacres of those times,' Hooker remarked, 'calling lunatics those few who, in advance of the generality of the then mankind, ventured to assert that they distinguished new colours.'<sup>2</sup> Hooker, who evidently positioned himself in this vanguard, knew, as he put it, 'perfectly well that sooner or later they [i.e., his discoveries which related the N-ray spectrum to types of human beings<sup>3</sup>] will become accepted facts.'<sup>4</sup> And he concluded by observing that 'the x rays are invisible but we all know that they are an existent fact. We have not yet reached the point of finality in rays'.<sup>5</sup>

In December, 1904, the Leconte prize of 50,000 francs was awarded to Blondlot by a committee of the Academy of Sciences, Paris, which included among others, Mascart, Berthelot, H. Becquerel and Poincaré. Although the committee did not emphasize Blondlot's investigations on N rays, it did enhance the respectability of their discoverer and thereby the credibility

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1. Hooker, 1904, p.1380.

2. Ibid., p.1381.

3. 'I have conducted,' Hooker reported, 'some 300 experiments to test this question of the human-ray spectrum and the extraordinary unanimity of the results is astounding.' Hooker claimed to have found that 'rays emanating from a very passionate man have a deep red hue; the one whose key-note in life is to be good and to do good throws off pink rays; the ambitious man emits orange rays; the deep thinker, deep blue... and so on'. (Ibid., p.1381.)

4. Ibid.

5. Ibid.

of his 'discovery'.<sup>1</sup>

In January, 1905, the Lancet informed its readers that a collection of Blondlot's papers, in English translation, was in the press, and opined that it 'should be of great interest as the existence of the "n" rays has been denied by a large number of scientific men in this country and on the continent'.<sup>2</sup> The book indeed appeared later in the year, bearing the title, 'N' Rays.<sup>3</sup> It contains, in addition to the papers which Blondlot submitted to the Academy of Sciences, Paris, notes and instructions for the construction of phosphorescent screens. The book claims that many of Blondlot's observations are simple and may be repeated without special apparatus; indeed, a small phosphorescent screen suitable for repeating some of the experiments, is attached to the book.

Under the heading, 'How the Action of "N" Rays should be observed', Blondlot gave the reader the following instructions:

It is indispensable in these experiments to avoid all strain on the eye, all effort, whether visual or for eye accommodation, and in no way to try to fix the eye upon the luminous source, whose variations in glow one wishes to ascertain. On the contrary, one must, so to say, see the source without looking at it, and even direct one's glance vaguely in a neighbouring direction. The observer must play an absolutely passive part, under penalty of seeing nothing. Silence should be observed as much as possible. Any smoke, and especially tobacco smoke, must be carefully avoided, as being liable to perturb or even entirely to mask the effect of the 'N' rays. When viewing the screen or luminous object, no attempt at eye-accommodation should be made. In fact, the observer should accustom himself to look at the screen just as a painter, and in particular an 'impressionist' painter, would look at landscape. To attain this requires some practice, and is not an easy task. Some people, in fact, never succeed.<sup>4</sup>

The book received a devastating review in one of the June issues of Nature. The reviewer, McKendrick, maintained that the controversy had arisen because the so-called proof of the existence of the rays depended, in his words, 'not on objective phenomena that can be critically examined,

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1. Stradling, 1907, p.124.

2. Lancet, 1(Jan.14, 1905), p.129.

3. Blondlot, 1905.

4. Ibid., pp.82-83 (emphases in the original).

but on a subjective impression on the mind of the experimenter, who sees, or imagines he sees, or imagines he does not see, a slight change in the degree of luminosity of a phosphorescing screen'.<sup>1</sup> McKendrick dismissed the photographic test as objective evidence since a slight difference in the time of exposure or in the method of development would readily account for the apparent contrast between the images of the spark with and without the influence of the alleged radiation.<sup>2</sup>

Yet, the experiments, the reviewer admitted, were well contrived and appeared to be accurately described. However, in every experiment, the reviewer reiterated, 'the ultimate test is the subjective one made on the mind of the observer as to whether a spot of slightly phosphorescent surface becomes more luminous or not'.<sup>3</sup>

In McKendrick's view, the special circumstances and training which the eye required in order to be able to observe the N-ray effects, seemed 'to be admirable arrangements for obtaining an illusive subjective impression!'<sup>4</sup>

McKendrick concluded his review of Blondlot's book by remarking that he did not for a moment reflect on the bona fides of the observers who had reported positive results; however, he held that they had been the subjects of either an illusion of the senses or a delusion of the mind.<sup>5</sup>

The Scientific American of October, 1905, agreed that the existence of N rays could be settled only by an objective demonstration of their effects. However, it was of the opinion that Blondlot's new automatic photographic technique in which the stability of the spark was constantly monitored, afforded such a demonstration. The journal stated categorically that 'these experiments [i.e., Blondlot's new photographic results] really

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1. McKendrick, 1905.

4. Ibid.

2. Ibid.

5. Ibid.

3. Ibid.

demonstrate the objective existence of the radiation.'<sup>1</sup> It thus concurred with Blondlot's claim that the application of the improved photographic method had only made the obtaining of the N ray effects more certain.<sup>2</sup>

However, during the years 1905 and 1906, French scientists took up the challenge which Wood had issued, and went to Nancy to confront Blondlot and his colleagues in their laboratories. The N-ray experts, however, evaded the request for more control experiments. Sensitivity of eyesight, rather than acceptance of objective criteria, remained the central issue for the proponents of the N rays. Blondlot in fact rejected such a request which was put to him in February, 1905. He was asked by the Revue Scientifique to assist in a test which involved the task of identifying from a collection of similar boxes those in which N-ray sources were placed. Blondlot responded by claiming that the N-ray effects were much too delicate for such an identification to be successful.<sup>3</sup>

As a new form of radiation, the N rays found their way, as it were, into the third supplement of the textbook, Cours de Physique de l'École Polytechnique,<sup>4</sup> published in 1906. The author, E. Bouty, expounded the subject as if it were a well-established scientific fact - part and parcel of our knowledge of radiation. In its review of the textbook, the journal Nature welcomed Bouty's exposition of N-ray radiation:

we have no wish to be dogmatic; there is certainly some evidence that M. Blondlot has been experimenting with objective, and not entirely with subjective, phenomena, and if this is so, experiments should not cease until the exact nature of these phenomena has been established.

However, the review was critical of Bouty's confidence:

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1. 'Photographic Records of the Action of N Rays', Sc. Amer., 93(Oct.14, 1905), p.299.
  2. Ibid. Cf., Stradling, 1907, p.127.
  3. Rev. Sci., 5 ser.5(1906), pp.135-36. Cf., Stradling, *ibid.*, p.189.
  4. Jamin, 1906.

'when M. Bouty... does not even hint that there is doubt, amounting to disbelief, in the minds of most of the leading physicists of the world in regard to this matter, we think that he is hardly doing justice to it.'<sup>1</sup>

The historical fact that papers concerned with the properties of N rays ceased to be published - they were probably rejected by the editors of respected journals of science - reflected the demise of the N-ray 'discovery'. Thus, a review of the N-ray affair, published in 1907 in the Journal of the Franklin Institute, declared that it 'aims to give a statement of the properties claimed to belong to the N rays, the  $N_I$  rays', and some other similar newly discovered rays, and to accompany it 'with a history of the investigations and of the decline of belief in the existence of these rays'.<sup>2</sup> The reviewer, G.F. Stradling, explained the demise quite rightly as due to the decline of belief in the existence of the N radiation. Characteristically, the discovery was taken up by the proponents of extra-sensory perception and other paranormal human phenomena in which people are always prepared to entertain beliefs.<sup>3</sup> We need not here elaborate the 'magical' resources that can be exploited in a discovery such as N rays. Rather, from the point of view of the present discussion, it is of interest to enlarge upon an analysis of the sources of error which, it appears, vitiated Blondlot's experiments, and - employing our classification of experimental errors - to elucidate the position of the critics with respect to that of the proponents of the rays.

At the outset of his analysis of the various explanations propounded to account for the success of the so-called discovery of the N rays, Stradling noted that three factors, essentially sociological, should be borne in mind.<sup>4</sup> In the first place, the discovery occurred at the right psychological

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1. Nature, 74(June 14,1906), p.148 (emphasis in the original).

2. Stradling, 1907, p.57.

3. Romilli, 1904.

4. Stradling, 1907,p.179.

moment for obtaining immediate belief in it. The newly discovered cathode rays, canal rays, X rays,  $\alpha$ ,  $\beta$  and  $\gamma$  rays, had conditioned the scientific community to the idea that some other kinds of radiation with astonishing properties might possibly exist. These discoveries had enhanced the search for radiation phenomena in perhaps a similar fashion to the way newly discovered particles initiate further search for more particles: the high energy physicist being conditioned to believe in the existence of new particles as Blondlot was with regard to radiation.<sup>1</sup>

Indeed, it appears that discoveries come in groups; as the Revue Scientifique remarked in 1904,

if the discovery of the N rays had been made 25 years ago, it would have encountered a scepticism that would have dwelt on the least sources of error. But the successive discoveries of... invisible forms of radiation... have turned the attention of scientists towards this kind of investigation and prepared the minds of all to accept them without surprise - even to await as a logical consequence the appearance of new kinds of radiation. And when the N rays were announced, there was everywhere a sort of satisfaction to see one step more made in such a triumphal march.<sup>2</sup>

Secondly, Blondlot's authority gave weight to his result; indeed, some French scientists declared explicitly that they had relied upon Blondlot's authority.<sup>3</sup> Even the Revue Scientifique stated that

at the very first, when M. Blondlot announced his resounding discoveries, the universal reputation of this eminent physicist permitted no mind to fail to accept as facts definitely won for science the published results of his remarkable initial experiments.<sup>4</sup>

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1. The case of G. LeBon's 'discovery' of black light bears some similar features to the case here presented. See Nye, 1974. For LeBon's view of N rays, see Rev. Sci., 2 ser.5(1904), pp.720-22.
  2. Rev. Sci., 2 ser. 5(1904), pp.551-52 (for the English translation see The Literary Digest, 29(Dec.17, 1904), p.840).
  3. 'Pour moi,' Poincaré said, 'j'ai la plus grande confiance en M. Blondlot, qui est un physicien très distingué et très habile.' (H. Poincaré, 1904.) Cf., Stradling, op.cit. Although Berthelot acknowledged that there had been points that need elucidation, he also believed in the existence of the rays largely on account of his personal confidence in Blondlot. (Rev. Sci., 2 ser.5(1904), p.590.) See also the view of Pellat. (Rev.Sci., 2 ser.5(1904), pp.590-91.)
  4. Rev.Sci., 2 ser.5(1904), p.547. Cf., Stradling, ibid.

In connection with Blondlot's authority and fame another factor, a third one, emerged, that is, the style of his papers. As McKendrick, the critical reviewer of Blondlot's book, admitted, the experimental evidence which Blondlot presented in his papers was adduced 'with a wonderful appearance of accuracy in detail, of the polarisation of the rays, of their dispersion, of their wavelength, and of other physical phenomena attributed to them. Prof. Blondlot's experiments,' McKendrick went on to say, 'are well contrived, and they give every appearance of being arrangements by which accurate data should be obtained'.<sup>1</sup> Thus, epistemological expectation, which was rooted in a certain psychological atmosphere, as well as authority and style, contributed to the immediate success, however short it was, which the N-ray discovery gained. Apart from these sociological factors, there were physical, physiological and psychological factors that bore upon this pseudo-discovery.

It is noteworthy that the critics, in their attempts to establish objective criteria for observing the effects of N rays, conceived explanations of the negative results which did not rely solely upon the observer's subjective impressions. That is, they sought either internal causes (e.g., 'blindness') to account for their inability to perceive the N-ray effects, or external causes (e.g., heat) which result in effects similar to those of the alleged rays. In comparison, the proponents of the rays resorted solely to subjective impressions, so much so that Blondlot advised the experimenter who wished to observe the N-ray effects, to play the role of an impressionist painter; he, of course, stopped short of instructing the observer to imagine the rays. However, it is misleading to say with the critics that this was wrong in that the ultimate test was subjective. By definition, an observation is a subjective process; the problem lay rather in the nature of the observation: it was simply unamenable to objective, or rather intersubjective

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1. McKendrick, 1905, my emphases.

criteria such as pointer reading. To observe the N-ray effects, one had to look for a small change of brightness which, of course, required remembering brightness – a very imaginative task indeed.

Applying our classification, we may note that the critics' suggestions for sources of error were not only based, as were those of the proponents, on the 'personal equation' type, but also on the 'observational error' type and on those of the 'assumptions concerning the actual set-up and its working'. Specifically, the suggested physical factors were of the latter class: heat radiation, decay of phosphorescence and change in the distance of the screen from the observer, could affect the observation in such a way that a change of brightness would be sensed.<sup>1</sup> Then, several physiological factors of the observational error type were suggested; namely, the tiring of the eye, change of the position of the image on the retina, change of accommodation, opening the eye to varying extents and periodic change in the curvature of the eye's lens, were considered as possible causes for obtaining change of brightness.<sup>2</sup> And, finally, the critics suggested psychological factors which could account for a biased connection between the observer's state of mind and the optical ability of his eyesight. These latter factors can be properly classified in the subclass of personal equation which subsumes, as I have defined it, all sources of error that have originated in idiosyncrasies of either psychological or physiological kinds. Thus, autosuggestions and expectancy,<sup>3</sup> reaction of the state of expectancy on the muscles of the eye and hypnotic sleep caused by sustained gaze at a small bright object, constituted such factors.<sup>4</sup>

On their part, the proponents of the N radiation explained failures to observe its effects as due to weak and inexperienced eyesight. 'The

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1. Stradling, 1907, pp.180-81.

2. Ibid., pp.182-85.

3. 'It is a great obstacle in experiments in N-rays that one does not know what change to expect.' (Hackett, 1902-1905, p.129.)

4. Stradling, 1907, pp.185-89.



effects to be observed,' it was argued, 'were only just within the range of visibility of all except those of considerable experience.'<sup>1</sup> Thus, those French scientists who were working closely with Blondlot, gained the necessary experience to confirm his results.<sup>2</sup> The inability to confirm the N-ray effects was therefore associated, from the proponents' point of view, only with errors of the personal equation type.

It is worthwhile to compare this case with the so-called discovery of the Mars canals. There, as here, the critics were relying in their critique not only upon sources of error of the personal equation type, but also on arguments pertaining to other classes of sources of error. We may therefore generalize this historical result and state that a critical outlook should not be based only on the personal equation subclass. It is, after all, in objectivity that science finds its strength. However, if the present thesis is aiming at a certain result, it is precisely this, namely, that inherent to the scientific method is the inability to find, or rather to found, categorical objectivity. And it is with amusement that we conclude this case by quoting the opening sentence of Hackett's 1904 paper on the photometry of N rays. 'It is rare in physical science,' Hackett stated, 'to find a divergence of opinion on a matter which can be subjected to experiment.'<sup>3</sup> He however conceded that 'perhaps never before was such a difference of opinion more justified than in the case of N rays'.<sup>4</sup> Our attempt is to disperse the myth of categorical experimental results, and to maintain the sceptical outlook by exploring the concept of experimental error.

We consider the N-ray affair an illustration of an experimental error of the third kind.

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1. Hackett, op.cit., p.135.

2. Ibid.

3. Ibid., p.127.

4. Ibid.

#### 5.4 J. Franck's and G. Hertz's experiment on the quantized spectrum of the atom's energy levels

'It appeared to me to be completely incomprehensible that we had failed to recognize the fundamental significance of Bohr's theory, so much so, that we never even mentioned it once in the relevant paper. It was unfortunate that we could not rectify our error... ourselves by clearing up the still existing uncertainties experimentally.'<sup>1</sup>

J. Franck

'We erroneously believed that... [what we had measured] was the ionization potential.'<sup>2</sup>

G. Hertz

'We know only too well that we owe the wide recognition that our work has received to contact with the great concepts and ideas of M. Planck and particularly of N. Bohr.'<sup>3</sup>

J. Franck

'The writer... cannot help but stand in admiration of the foundations of discharge theory as conceived by Townsend, whose importance and value are in no sense dimmed by the fact that after forty years it is found in some points to be inadequate... In view of the fact that for many years Townsend did not accept the Franck and Hertz experiments and in later writings has never clearly retracted the theory, and in view of the authoritativeness of his work and its inclusion in the old form in many current texts, it is necessary at this point once and for all clearly to indicate the fallacy... The observed agreement was... entirely illusory through the fortuitous agreement of the equation with a very limited stretch of curve.'<sup>4</sup>

L.B. Loeb

The study of collision processes in gases falls naturally into two categories according to whether they are treated in bulk or as single events. To the former category belong the glow and arc discharges, the processes of diffusion and mobility, and other allied phenomena which occur when the pressure is high and the collisions numerous. Such phenomena are amenable to statistical analysis. In contrast, the study of single collisions can be pursued when the pressure is sufficiently low so that the mean free paths of the colliding bodies are considerably large with respect to the dimensions of the apparatus.<sup>5</sup>

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1. Franck, 1965, p.106.

4. Loeb, 1939, pp.337, 361.

2. Hertz, 1965, p.124.

5. Arnot, 1946, p.v.

3. Franck, 1965, p.108.

Historically, the studies leading to the investigation of the phenomenon of ionization by electron-impact gathered momentum with the research in 1900 of J.S. Townsend (1868-1957) into the 'multiplication of charges' in a gas subject to an electric field.<sup>1</sup> Since then, through almost three decades, Townsend was trying to establish a theory of ionization of gases by collision on the basis of his experiments which belong to the first category.

However, Townsend's experiments, though well adapted to the study of discharge and other bulk phenomena like diffusion and mobility, 'were not suitable', as G.P. Thomson remarked, 'for finding out how the act of ionization occurs. Progress here was left for Franck and Hertz working in Göttingen in 1914, whose experiments were a valuable help to the establishment of the quantum theory'.<sup>2</sup> The Franck and Hertz experiment was essentially a study of single collisions: an experiment of the second category.

The contrast between the so-called success of Franck and Hertz and the failure of Townsend illustrates the distinction between an error on the level of interpretation - the theoretical conclusions - and an error on the methodological level - the background theory. The former error originates in a misapprehension of significant results, whereas the latter may arise out of an experimental method which is unsuitable for the purpose of a certain study: it allows one to arrive at certain experimental results which may be interpreted successfully to a limited extent; however, it cannot reveal the exact physical features of the phenomenon at stake.

In 1900, Townsend published a note in Nature<sup>3</sup> giving a

brief description of some experiments which showed that negatively charged ions, moving through a gas, produce other ions, although the force acting on them is very small compared with the force necessary to produce the ordinary vacuum-tube or spark discharges.<sup>4</sup>

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1. Engel, 1957, p.261; Rayleigh, 1942, pp.115ff.

2. G.P. Thomson, 1964, p.103.

3. Townsend, 1900, pp.340-41.

4. Townsend, 1901, p.198.

Townsend explained that the initial

negative ions produce others by collisions with the molecules of the gas when the electric force is increased, the new negative ions thus produced having the same property as the negative ions produced initially by the rays.<sup>1</sup>

Townsend contrasted this process with the observation that when the electric force is too small to trigger off the above phenomenon, and the ions produced by the rays are collected on the electrodes, the resultant current is practically independent of the electric force.<sup>2</sup> It was only at a later date that Townsend identified the colliding bodies with electrons.<sup>3</sup>

It should be noted that at the turn of the century the concept of ionization of gases was not developed to any appreciable degree. 'It was thought,' observed von Engel in his obituary of Townsend, 'that in order to ionize air an ionization energy of about 175eV was required. Townsend derived from his own work and previous work by Stoletow<sup>4</sup> that very much lower energies are sufficient to stimulate this process'.<sup>5</sup> In fact Townsend revolutionized this field of study by introducing the concept of 'ionization by collision'.<sup>6</sup>

Having observed the phenomenon of ionization by collision, Townsend proceeded to determine quantitatively the specific value of the energy which is required to bring about such a process in a particular gas. This attempt was very important since the incorporation of its results with any theory of the structure of the atom could test the validity of such a theory. However, Townsend's results were never conclusive and always in disagreement with

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1. Ibid., p.205. In Townsend's early experiments a number of ions were generated initially in the gas by some external source, such as Röntgen or Becquerel rays. (Ibid., p.204. Cf., Townsend, 1910, p.3.)
  2. Townsend, 1901, p.204.
  3. Engel, 1957, p.261. Cf., Loeb, 1939, p.341.
  4. Townsend, 1900, p.341.
  5. Engel, 1957, p.261.
  6. Loeb, 1939, p.341.

the results obtained by other experimenters. It is ironic that a great experimenter like Townsend should conclude after many years of work that

there are considerable differences in the values of these potentials [the ionization and excitation potentials] deduced from experiments on electric currents by different observers. The values which are usually adopted are those which are in agreement with the values deduced from Bohr's quantum theory of spectral lines.<sup>1</sup>

With a tone of resignation Townsend seems here to question implicitly the role of experiment in science.

Essentially, Townsend's method involved the study of the rate of growth of currents in a parallel-plate apparatus as the potential increased; a small initial current is provided by an external source such as ultra-violet light falling on one electrode. The electric fields and the pressures are such that the electrons assume sufficient energy to release more electrons from the gas molecules as a result of collisions. The distance,  $x$ , between the plates is made continuously variable; the pressure,  $p$ , and the electric field,  $X$ , are kept constant in any one set of measurements. The resultant current-distance curve is then exponential for a certain range of potentials. In other words, as Townsend stated, 'it has been found experimentally that the number of negative ions  $n$  that arrive at the positive electrode is given by the formula  $n = n_0 e^{\alpha x}$ ;<sup>2</sup>  $n_0$  being a constant (the initial number of negative ions), and  $\alpha$  being the number of ion pairs generated by one ion (electron) per centimetre of its path.<sup>3</sup>

Having obtained  $\alpha$  for a limited range of potentials and pressures,<sup>4</sup> Townsend observed that

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1. Townsend, 1947, p.118. Bijl pointed out in 1917 that 'the experimental determination of the ionization voltage is not a simple matter. The observed ionization at voltages below that required by the Bohr theory does not necessarily invalidate this theory'. (Bijl, 1917b, p.556.)
  2. Townsend, 1915, p.274. Cf., Atkinson, 1928, a, pp.335-36.
  3. Townsend, *ibid.*, p.264.
  4. *Ibid.*, pp.276-79.

the values found for  $\alpha$  for various forces and pressures may be recorded in a simple manner, as it was observed that when the points whose co-ordinates are  $\alpha/p$  and  $X/p$  are marked on a diagram they all lie on one curve. The variables  $\alpha$ ,  $X$ , and  $p$  are therefore connected by an equation of the form  $\alpha/p=f(X/p)$ .<sup>1</sup>

Thus, according to Townsend, the proper parameter is not the electric field but rather, as required by the theory he had developed, the field reduced to unit gas pressure.

Triumphantly, Townsend concluded that

this connection between the three variables [that is,  $\alpha$ ,  $X$ , and  $p$ ], which was deduced from the numbers found experimentally, affords the strongest evidence in support of the theory of ionization by collision. For if the number of molecules  $\alpha$  ionized by an electron, or negative ion, per centimetre of its path depends on the velocity with which it collides with the molecule, it may easily be seen that a relation of the above form must hold between the quantities  $\alpha$ ,  $X$ , and  $p$ .<sup>2</sup>

However, J. Franck (1882-1964) and G. Hertz (b.1887) were not impressed by this result; they thought that Townsend's hypothesis on the kind of collisions between slow electrons and atoms 'differed', as Franck put it, 'from the reality'.<sup>3</sup> Franck and Hertz decided therefore to study afresh the process of collision in gases by means of a systematic examination of the elementary processes occurring in collisions between slow electrons, atoms and molecules. At the outset, they chose for their study gases without electron affinity, that is inert gases and metallic vapours, which cannot form negative ions and as a result of that, the electrons are 'left' to move freely. They expected that in such gases the motion of electrons would obey laws of a particularly simple kind.<sup>4</sup>

In that pursuit, as Franck reported, 'we had the experiences and techniques to support us, which men like J.J. Thomson, Stark, Townsend, and

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1. Ibid., p.279.

2. Ibid., p.280. On Townsend's theory of ionization see Stranathan, 1942, pp.11-23.

3. Franck, 1965, p.99.

4. Ibid., pp.98-99. Cf., Hermann, 1971, p.77. For an extensive bibliography see Franck and Hertz, 1919, p.143.

in particular, however, Lenard, had created, and also had their concept of the free path-lengths of electrons and the ionization energy, etc., to make use of'.<sup>1</sup> Indeed, the experimental technique of Lenard which Franck and Hertz adopted with important modifications, proved to be very fruitful in their research.<sup>2</sup>

Lenard's method, unlike that of Townsend, is direct and designed to explore the various effects arising from the incidence of light of short wavelength on metal surfaces. His paper, 'Light Electric Effect',<sup>3</sup> gives the results of experiments when the metal is in a vacuum, and when in a vessel filled with gas. Lenard observed that the 'carriers of electricity' which were released from the metal surface and shot into a space filled with gas, made it conductive; he then proceeded to determine the smallest velocity of these 'carriers' (i.e., electrons) at which, as he thought, ionization occurs.

In Lenard's method, electrons were generated by the action of ultra-violet light on a metal plate U, and accelerated up to a gauze E - a distance of 1.45cm. from U - by a potential  $V_1$  applied between U and E. Having passed through E, the electrons were retarded by an opposing potential  $V_2$  applied between E and an insulated ring R which was placed parallel to U and E in a distance of 3cm. from U. As  $V_2$  was always greater than  $V_1$ , no electrons from U could reach R. The pressure of the gas in the apparatus was about  $10^{-2}$  mm. of Hg so that the mean free path of an electron was comparable to the distance between U and R. Lenard suggested that the positive current which was recorded at R set in as soon as the energy of the electrons was sufficient for ionizing the gas molecules.<sup>4</sup>

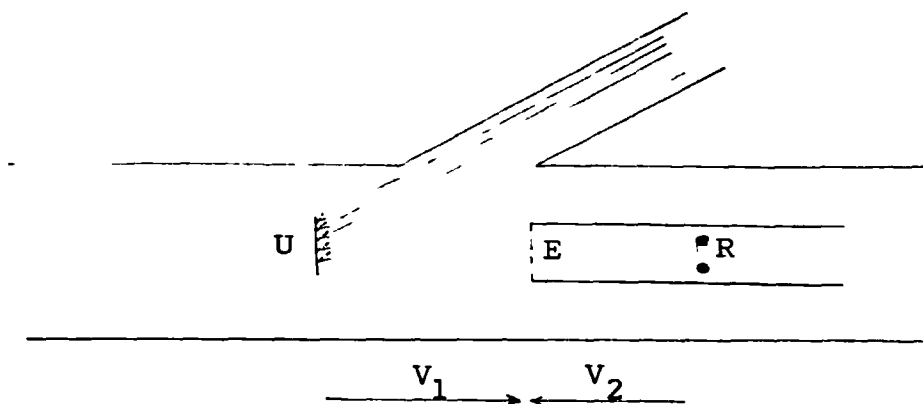
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1. Franck, 1965, p.99.

2. Ibid., pp.102-3.

3. Lenard, 1902; this is a continuation of an earlier paper: Lenard, 1900.

4. Lenard, 1902, pp.188-98. Cf., Sci. Abs.,5(1902), p.784 art. 1883; Townsend, 1915, p.296; Arnot, 1946, pp.13-14.



However, up to 1913 Lenard's method had always given an unsatisfactory result: an apparent ionization potential of 11 volts for all the gases Lenard experimented with, namely, air, hydrogen and carbon dioxide. As it was thought at that time that the positive current was entirely due to positive ions, Lenard arrived at the surprising conclusion that 11 volts is the ionization potential of these gases.<sup>1</sup>

It may be remarked in passing that Lenard's result was perhaps fairly precise but definitely inaccurate. Due to impurities, Lenard always measured the ionization potential of mercury vapour.<sup>2</sup> Furthermore, he was not aware of the photoelectric effect which took place as a result of newly created sources of radiation, that is, the gas molecules reaching their excited states. It was only as late as 1917, that this shortcoming of the Lenard method was acknowledged.<sup>3</sup> As we shall see, this criticism had a direct bearing on the Franck and Hertz experiment.

However, Lenard's method had an important feature which did not escape the critical eyes of Franck and Hertz. This method was capable of lending a direct decision regarding the transfer of energy by an inelastic impact. In their early experiments on collision processes in gases, Franck and Hertz claimed to show, with an accuracy then estimated at 1/3 to 1/10 volt, that at low potentials - hence, low velocities - 'no loss' of energy occurred

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1. Loeb, 1938, p.250; Arnot, *ibid.*, p.14; Franck, 1965, p.102.

2. Loeb, *ibid.*; Bishop, 1917, p.251.

3. Bijl, 1917, a.



on impact: the loss involved in an elastic collision being of the order of 1/1000 volt. It was immediately evident that these experiments were concerned only with possible inelastic losses.<sup>1</sup>

Franck and Hertz explained that they were not concerned with the small difference between 'no loss' and 'elastic collision'. The position they took was that, to quote Franck and Hertz, 'if the collisions were strictly elastic, the electron, as is easy to see, would lose in helium, for example, roughly 1/4000 of its energy at each encounter; such a loss cannot be established by our methods'.<sup>2</sup> Collisions without loss of energy were simply experimentally indistinguishable from elastic collisions. 'Only for high pressures, that is, with the occurrence of many thousands of collisions', Franck remarked, 'can the energy loss corresponding to elastic collision be demonstrated.'<sup>3</sup> That was an important physical approximation; it enabled Franck and Hertz to focus their experiments upon the borderline between elastic and inelastic impact.<sup>4</sup>

Anticipating the error in Franck's and Hertz's experiment, we may note that it did not originate in this physical approximation, as Townsend wanted us to believe,<sup>5</sup> but rather from the so-called fact that 'the new method of measuring the ionization potential rests on the fact that the ionization energy is the maximum kinetic energy that electrons can have and still be reflected without energy loss after numerous collisions with the gas molecules'; and from the theoretical conclusion that 'the frequency of a definite proper vibrational mode of an electron multiplied by the

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1. Franck and Hertz, 1913. Cf., Atkinson, 1928,a, p.346.
  2. Franck and Hertz, 1914,a, p.19. Cf., Atkinson, *ibid*.
  3. Franck, 1965, pp.101-2. That was indeed the Townsend type of experiment.
  4. Holton, 1961, p.808 (excerpt from a recording of J. Franck). Franck relates that they hoped that only one spectral line would appear on this borderline. (*Ibid*.)
  5. *Infra*, pp. 273-75.

constant  $h$  is equal to the energy required for ionization'.<sup>1</sup>

On the basis of Lenard's method, Franck and Hertz developed, in 1914, a device which was the first to give definite results in the field of collision processes in gases. Their method can be generally characterized as the study of impact phenomena by electrical devices and the confirmation of the values thus derived by spectroscopic method. Their apparatus for the detection of inelastic collisions is shown in the following schema:



The gas under study is contained in a glass envelope at low pressure. A three-electrode structure consisting of a thermionic filament F, a grid G, and a plate P is mounted in this envelope. The electrons are accelerated from the filament F to gauze G by a variable potential  $V_a$  and retarded after passing through G by a small potential of about 0.5 volt. The pressure in the apparatus is about 1mm. of Hg, so that the mean free path of an electron is much smaller than the distance F to G, but equal to, or greater than, the distance G to P. Normally, the current to P will increase with  $V_a$ , the emission from F not being saturated. If, however,  $V_a$  just exceeds a critical value, some of the electrons will lose nearly all their energy before reaching G and will be stopped by the back potential. Thus the current will fall with increasing  $V_a$ , till  $V_a$  becomes equal to the critical energy plus the retarding potential, when it will rise again. There will be a further fall at twice the critical potential and so on. The current recorded at P is plotted against the accelerating potential  $V_a$ , and the

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1. Franck and Hertz, 1914,b, pp.458, 464; for an English translation see Boorse and Motz, 1966, pp.771, 777 (my emphasis).

curve thus obtained exhibits a series of regularly spaced peak values. The difference between the maxima gives the critical potentials. The values thus obtained are then checked against the values derived from spectroscopic observations in accordance with the equation  $h\nu = Ve$ .<sup>1</sup>

In their paper, 'Collisions between Electrons and Mercury Vapour Molecules and the Ionization Potential of Such Molecules', Franck and Hertz reported their findings and concluded:

- '1. We have demonstrated that the electrons in mercury vapour suffer elastic collisions with the molecules up to a certain critical speed.
2. We have described a procedure for measuring this critical speed accurately up to a tenth of a volt. It is equal to the speed acquired by an electron that falls through a potential difference of 4.9 volts.
3. We have shown that the energy of a 4.9-volt electron beam is exactly equal to the quantum of energy associated with the mercury resonance line  $253.6\mu\mu$ .
4. We have discussed why, in the transfer of the energy from the 4.9 volt beam to the mercury molecule, some of the collisions lead to ionization, so that it appears that the ionization potential of mercury is 4.9 volts. Another part of the collisions appears to stimulate the emission of radiation and we surmise that this corresponds to the line  $253.6$ .'<sup>2</sup>

It is perhaps due to their 'success' in recording only one critical potential that they reached such 'conclusive' results. They did not record higher critical potentials because the mean free path of the electrons is so short that the electrons have little chance, in the Franck and Hertz arrangement, of gaining more than 4.9 volts energy before colliding with the gas molecules. And secondly, the sensitivity of their device was not

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1. Franck and Hertz, *ibid*; Franck and Hertz, 1914,c (for an English translation see Haar, 1967, pp.160-66). Cf., J.J. Thomson and G.P. Thomson, 1933, Ch.3 (The Collisions of Electrons with Gas Molecules); Arnot, 1946, pp.14-16.
  2. Boorse and Metz, 1966, p.778.

sufficient to detect the inelastic collision at 4.66 eV which leads to the excitation of a metastable state. In retrospect, Hertz remarked that that 'was in reality a fortunate circumstance since we would not have been able at that time to relate this energy quantum to the atomic spectrum of mercury'.<sup>1</sup>

However, Franck and Hertz regarded the first inelastic impact as an ionization process; they thus believed that the spectral line 2,537.7 Å was emitted at 4.9 volts as a result of the ionization of the mercury molecules. They were wrong in this interpretation; in other words, what they measured was not what they thought they were measuring. In fact, their device could not distinguish between excitation and ionization potentials; it only recorded the occurrences of inelastic collisions. The accepted view today is that 4.9 volts (more accurately, 4.89 volts) is not the ionization, but rather the first excitation potential of mercury.

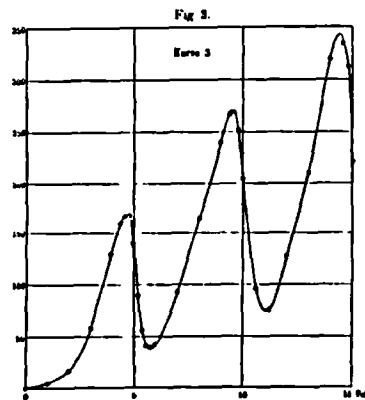
Let us examine more closely what is now believed to occur in the Franck and Hertz set-up:

1. As long as the accelerating potential  $V_a$  is smaller than the retarding potential  $V_r$ , the current recorded at P is zero.
2. When  $V_a$  is greater than  $V_r$ , the current rises until  $V_a$  equals the first excitation potential  $V_e$ .
3. At that moment the electrons in the neighbourhood of the wire mesh G suffer inelastic collisions and induce excitation. Since the electrons lose almost all their energy they cannot overcome  $V_r$  and the current therefore falls. However, it does not fall to zero because of the photoelectric effect.
4. The current will rise again when  $V_a$  becomes equal to the excitation potential  $V_e$  plus the retarding potential  $V_r$ .

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1. Hertz, 1959, p.39. Quoted by Hermann, 1971, pp.78-79.

5. With the rise of  $V_a$  the region where the electrons suffer inelastic collisions moves inwardly away from the mesh G towards the electrode F.
6. The electrons thus suffer inelastic collisions away from the mesh G and, therefore, fall on their way to G through a potential that is equal to the difference between  $V_a$  and the excitation potential  $V_e$ .
7. As soon, however, as  $V_a$  equals twice the excitation potential, the electrons in the neighbourhood of G suffer inelastic collisions for the second time (the first having occurred half-way from F to G).
8. Stage 3 is now repeated, and the current sinks to a level higher than the previous minimum - the photoelectric effect being more intense.
9. This same phenomenon recurs whenever  $V_a$  is an integral multiple of  $V_e$  (one has, however, to bear in mind the work function of the filament F). One obtains therefore a curve which has maxima of increasing size and are spaced at just the excitation potential.



10. The mean free path of the electrons is so short that they have little chance, in this arrangement, of gaining more energy than  $V_a = V_e$  before colliding inelastically at the first excitation potential  $V_e$ . However, few electrons may succeed in reaching higher values of energy and indeed induce excitation at higher critical potentials; but these events are masked by the dominant process which occurs at the first excitation potential.

Following the erroneous conclusion of Franck and Hertz, several experimenters either confirmed it or generalized the conclusion while emphasizing its importance. Just after the publication of Franck's and Hertz's

experiment, Newman reported that he had obtained the same result.<sup>1</sup> Later, in 1915, McLennan and Henderson emphasized that

this result constitutes a new and most interesting application of the quantum theory, for... if the vapour of an element can be shown to be capable of exhibiting a single-line spectrum, the frequency of this single spectral line may be used to deduce the minimum amount of energy required to ionize the atoms of that element.<sup>2</sup>

However, they found that apparently in mercury vapour the single line  $\lambda = 2536.7 \text{ \AA}$  was emitted alone up to a value of voltage slightly greater than 10 volts and the many-lined spectrum of mercury suddenly appeared if the voltage was increased to 12.5 volts. They pointed out that this value is of the same order of magnitude as that calculated from the quantum relation,  $Ve = h\nu$ , when the frequency taken is that of the shortest wavelength of the Paschen combination series of the mercury spectrum (10.5 volts); whereas the line  $\lambda = 2536.7 \text{ \AA}$  is the first or longest wavelength in this same series. Interpreting their results as indicating possibly a second type of ionization, they could still adhere to the conclusion of Franck and Hertz.<sup>3</sup> Clearly, a new theoretical framework was needed in order to acknowledge the error of interpretation Franck and Hertz had, so to speak, committed.

In his paper, 'On the Quantum Theory of Radiation and the Structure of the Atom',<sup>4</sup> Bohr succeeded in providing a theoretical framework which could incorporate these results in a new way and thus confirmed an error in Franck's and Hertz's conclusion. Bohr clearly recognized the importance of this experiment: it provided a possible experimental confirmation for his theory.

Bohr suggested that what Franck and Hertz had taken to be a stream of positive ions from the gas to the plate P, could be interpreted differently:

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1. Newman, 1914.

2. McLennan and Henderson, 1915, p.485.

3. Ibid.

4. Bohr, 1915.

it could have been a current of photoelectrons produced in the plate by the resonance radiation of the gas, and driven off by the high negative potential. He thus maintained that the Franck and Hertz experiment 'may possibly be consistent with the assumption that this voltage [4.9 volts] corresponds only to the transition from the normal state to some other stationary state of the neutral atom'.<sup>1</sup> Furthermore, on the basis of his theory, Bohr calculated the ionization potential and arrived at the conclusion that it should be 10.5 volts. He therefore concluded that if these

considerations are correct it will be seen that Franck's and Hertz's measurements give very strong support to... [my] theory;

he, however, added that

if, on the other hand, the ionization potential of mercury should prove to be as low as assumed by Franck and Hertz, it would constitute a serious difficulty for... [my] interpretation of the Rydberg constant, at any rate for the mercury spectrum, since this spectrum contains lines of greater frequency than the line 2536.<sup>2</sup>

When Franck and Hertz re-examined their position in 1916, they were reluctant to accept any change in their interpretation of the experiment and thus put Bohr's theory in question.<sup>3</sup>

However, in the light of Bohr's theory, McLennan was led to question whether ionization really took place at 4.9 volts or only at more than 10 volts when the many-lined spectrum was emitted.<sup>4</sup> It may be recalled that McLennan had suggested, in 1915, a second type of ionization in order to accommodate his results with those of Franck and Hertz. But, in 1916, he argued that Bohr's theory would

predicate but one type of ionization for atoms. By applying the theory to the matters discussed... it would appear that atoms in the state to emit a single-line spectrum could not be said to be ionized. It would follow, then, that if Bohr's theory of the origin of radiation

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1. Ibid., pp.410-11.

2. Ibid., p.411.

3. Franck and Hertz, 1916; Heilbron, 1977, pp.76-77.

4. McLennan, 1916.

be correct, the interpretation placed by Franck and Hertz on the results of their direct investigation of<sub>1</sub> the ionizing potentials for mercury atoms cannot be the correct one.

McLennan was thus the first experimenter who, on the basis of Bohr's theory, questioned the validity of Franck's and Hertz's interpretation.

Nevertheless, McLennan remained sceptical with regard to the validity of Bohr's interpretation; he could not find the other strong lines in the Paschen series, e.g., the wavelength  $\lambda=1849.6 \text{ \AA}$ , which he correctly thought should appear, according to Bohr's theory, with a potential of 10 volts.<sup>2</sup> However, later on, in the same year, he studied the conductivity of flames in which mercury vapour was present in a state of emitting only the line  $\lambda=2536 \text{ \AA}$ . In this experiment he arrived at the conclusion that 'mercury vapour which is fed into the flame of a Bunsen burner is ionized, and the radiation from the vapour consists of light of wavelength  $\lambda=2536.72 \text{ \AA}$ '.<sup>3</sup> McLennan concluded that, on the basis of Bohr's theory,

if an atom emits light of but one wavelength it cannot be said to be ionized. The results of the experiments with mercury vapour would indicate that the theory is invalid, for the evidence goes to show that the radiation emitted by the atoms of the vapour was entirely monochromatic, and at the same time it supports the view that under these circumstances the vapour was ionized.<sup>4</sup>

However, other physicists remained critical of the experiment at stake, and raised objections on experimental as well as theoretical grounds.

From the experimental point of view, Goucher noted that

the exact point at which ionization set in was rendered doubtful because of a lack of homogeneity of velocity among the electrons. This lack of homogeneity arose in consequence of two causes in the cases where the hot filament was used, viz., the IR drop of potential along the wire, and the initial velocity of the electrons themselves due to the high temperature of the filament.<sup>5</sup>

He therefore suggested minimizing these two sources of error in order to get an accurate value of the ionization potential of mercury vapour.<sup>6</sup>

He thus implicitly considered Franck's and Hertz's interpretation correct.

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1. Ibid., p.311.

4. Ibid.

2. Ibid.

5. Goucher, 1916, p.561.

3. McLennan and Keys, 1916, p.607.

6. Ibid., p.562.



With regard to the theoretical aspect of the Franck and Hertz experiment, Tate remarked in 1916 that the experimental evidence of Franck and Hertz had not definitely proved that mercury vapour was ionized by collision with an electron possessing an energy of 4.9eV. All that the experiment had demonstrated, he maintained, was that 'the collisions become inelastic at that point. It is readily conceivable', he suggested, 'that the energy lost by the colliding electron merely goes over into energy of agitation of the electrons bound in the atom and not necessarily into completely separating one or more electrons from the atom'.<sup>1</sup> Tate thus found it necessary to determine the value of the critical potential at which the energy that the electrons give up is emitted as radiation of many frequencies corresponding to the many-lined spectrum of mercury; and, furthermore, to establish whether or not ionization takes place at this point. In his investigation, Tate arrived at the following conclusions:

- '(1) A marked ionization occurs in mercury vapour when the velocity of the colliding electrons reaches a critical value of 10.0 volts (possible error about .3 volt). This is very nearly the value (10.2 volts) to be expected on the basis of Bohr's theory of the atom as McLennan has pointed out.
- (2) The energy lost by the electrons at these collisions is radiated out as the many-lined spectrum of mercury.
- (3) Although ionization of mercury vapour at 4.9 volts is not definitely disproved it is certainly much less complete than the ionization taking place at 10.0 volts.'<sup>2</sup>

Though Tate's results were accurate, they were not conclusive; the difficulty whether the critical potential at 4.9eV was ionization or excitation potential still required clarification. It was Van der Bijl who, by tracing the problem to its origin, i.e. Lenard's method, provided in

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1. Tate, 1916, p.686.

2. Ibid., p.687.

1917 the much needed elucidation.

Van der Bijl critically observed, as Tate had done, that the inelastic collision which occurs when the electron acquires an energy corresponding to 4.9 volts,

'merely means that when the electron strikes a molecule with 4.9 volts energy, it gives up its energy to the molecule, but whether this energy,' he argued, 'is transformed into ionization as well as radiation, as Franck and Hertz assumed, or into radiation alone, cannot be decided from their experiment.'<sup>1</sup>

Van der Bijl questioned therefore 'whether the positive charge acquired by the electrometer in the experiments... was actually due to ionization'.<sup>2</sup> He observed that

'when the electron acquires energy equivalent to 4.9 volts in the case of mercury, for example, the corresponding line 2536 is emitted. There is thus created a source of ultra-violet light in the tube. This light falls on the plate which is connected to the electrometer and so', he suggested, 'can emit electrons from the plate photo-electrically, thus causing the electrometer to charge up positively. It is just possible that this is what was measured. Such a possible effect was not excluded from any of the experiments published.'<sup>3</sup>

In the light of his suggestion, Van der Bijl explained that the very slow increase in the positive current, obtained for voltages between 5 volts and 10 volts, might have been due to the comparatively small photo-electric effect; whereas the rapid increase in the positive current for voltages above 10 volts could be due to intense ionization which is super-imposed upon the photocurrent.<sup>4</sup>

Van der Bijl concluded therefore that if

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1. Bijl, 1917,a, p.173.

2. Ibid., p.174.

3. Ibid.

4. Ibid. However, it should be noted that had the experimenters tried to determine this process by the resultant shape of the curve, they would not have succeeded since the number of radiating sources and the positive ions produced would both be proportional to the number of impacts. But in any event, as Van der Bijl observed, experimenters who employed Lenard's method at that time did not consider such a process, though Bohr, as we have seen, had hinted at it. (Ibid.)

it should be found that such a photo-electric effect actually is present to an extent great enough to make itself felt, there would be strong reason for believing that what we might call the ionizing potential of mercury vapour is more nearly represented by 10.27 than by 4.9 volts. This would correspond to the limiting line of the series.

This was an important suggestion which Davis and Goucher confirmed that very year.

Davis and Goucher realized that Bijl's explanation could account for almost all the findings in this field of study, and that the flame conduction experiments of McLennan were open to the same criticism as those employing the direct method of Lenard. They thus found it 'highly desirable to determine whether or not the effects occurring below 10.4 volts were due to ionization or to the emission of ultra-violet light from the bombarded atoms, and whether or not positive ionization actually took place at 10.4 volts'.<sup>2</sup> To test these suggestions Goucher proposed to introduce a second gauze into the apparatus employed in the Franck and Hertz method. His apparatus was the first device which was capable of distinguishing excitation from ionization potential. Clearly, Bijl's suggestion served as a theoretical background to this modification of the Franck and Hertz method.<sup>3</sup> Using this new method, Davis and Goucher arrived at the following conclusions:

'(a) Radiation is emitted without ionization at an impact voltage of 4.9 volts. This voltage corresponds to the frequency of the first line  $\lambda=2536.7 \text{ \AA}$  of the Paschen combination...

(b) An increase in the intensity of the radiation takes place at an impact voltage of about 6.7 volts. This voltage corresponds to the frequency of the... line ( $\lambda=1849 \text{ \AA}$ )...[This is the spectral line McLennan sought.]

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1. Ibid., pp.174-75. Van der Bijl explicitly stated that such a phenomenon would be capable of explanation on Bohr's theory; however, he raised 'a question as to whether such a thing as a definite ionizing potential of an element exists at all'. (Ibid., p.175.)
  2. Davis and Goucher, 1917, p.102.
  3. Ibid., pp.102-11. Cf., Arnot, 1946, pp.18-19; for further improved methods see pp.16-24.

(c) Ionization by impact, without an apparent increase in radiation, occurs at an impact voltage of about 10.4 volts. This voltage corresponds to the head or shortest wavelength of this same principal series.'<sup>1</sup>

These results confirmed Van der Bijl's suggestion and thus established the validity of Bohr's theory with regard to these processes. Consequently, Franck's and Hertz's interpretation was found to be in error.

Franck's and Hertz's experiment constitutes a case where an error originated solely in the interpretation. It was neither the method nor the physical approximation, but rather the theoretical considerations in comprehending the observations that gave rise to an error. Franck and Hertz measured, it is now believed, a real physical quantity: the first excitation potential of mercury; however, they thought it to be an ionization potential and therefore erred in their interpretation of the observational results.<sup>2</sup>

Though the physics community acknowledged the Franck and Hertz experiment and, once corrected, approved of its conclusions, Townsend persisted in insisting that his results gave the correct values of the ionization potentials. 'For many years,' Loeb observed, 'Townsend did not accept the Franck and Hertz experiments and in later writings has never clearly retracted the theory.'<sup>3</sup>

In 1923 Townsend noted that experiments which had been carried out according to his suggestions

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1. Davis and Goucher, *ibid.*, p.110. Davis and Goucher suggested that 'the definiteness of the results are due to the fact that the impacts in mercury vapour are perhaps completely elastic. That is, an electron loses no energy at impact with a mercury atom, unless either radiation or ionization is produced, in which case the entire energy of the electron goes into the radiation or the ionization, and none is absorbed by the atom'. (*Ibid.*)
  2. It is noteworthy that the recognition of their error arose from experiments based on the Lenard method, the very method they had employed.
  3. Loeb, 1939, p.361.

are not in agreement with the modern views held by some physicists as to the mode of development of currents in monatomic gases, or with the determinations of resonance potentials or ionization potentials as found by experiments where the electrons from a hot filament pass through a gauze between two fields in which the electric forces act in opposite directions.<sup>1</sup>

Townsend observed that according to the modern theory

the curve representing the currents in terms of the potentials should... be in the form of a series of well-defined steps with angular points at the top and bottom of each step; and when experimental results conflict with this view, the discrepancies are attributed to impurities.<sup>2</sup>

Since he had not obtained this result, Townsend set himself to examine this explanation more closely by 'finding a large number of points on the current-potential curve', and by taking 'every precaution to eliminate impurities'.<sup>3</sup> He reported, however, that his experiments of the parallel-plates type demonstrated that 'the current-potential curves do not show any steps... where the current increased with the potential  $V$ , which was proportional to the distance  $x$  between the plates, by the factor  $e^{\alpha x}$ '.<sup>4</sup>

Townsend thus remarked that 'there is a considerable difference between the above conclusions and some of those deduced from the valve experiments'.<sup>5</sup> However, his theory was completely incapable of accounting for the phenomenon of abrupt decrease in current with increasing potential, observed in the Franck and Hertz experiment.<sup>6</sup>

A new set of the Townsend experiments were carried out a year later. Once again he was unable to reconcile the results of the experiments with the

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1. Townsend, 1923, p.1071.

2. Ibid.

3. Ibid.

4. Ibid., pp.1071-72.

5. Ibid., p.1079. According to Townsend the apparatus used in the Franck and Hertz experiments 'resembles the three-electrode valve used in wireless telegraphy; and this method of investigation may be referred to as the valve method in order to distinguish the results from those obtained by measuring the conductivity between parallel plates'. (Ibid., p.1071.)

6. Atkinson, 1928, a, pp.347-48.

quantum theory, and found that 'the original theory of ionization by collision which was given many years ago affords a satisfactory explanation of the experiments'.<sup>1</sup> As in 1923, 'every precaution has been taken to have the gas free from impurities'.<sup>2</sup> From these experiments Townsend concluded that 'electrons moving with velocities corresponding to resonance potentials very rarely lose their energy when they collide with molecules'.<sup>3</sup> Furthermore, 'the kinetic energy of the electron must be at least twice the energy required to ionize a molecule in order that ionization may actually take place to an appreciable extent'.<sup>4</sup> Townsend stressed that these results differed 'from the conclusions to which physicists have been led by applying the quantum theory';<sup>5</sup> and held that 'the evidence [for these conclusions] is not very convincing'.<sup>6</sup> But that was not the view of the physics community. In 1925, the Physics Nobel Prize was awarded to Franck and Hertz.

The Nobel Committee for physics singled out the experimental work of Franck and Hertz, citing their discovery of 'the laws governing the impact of an electron upon an atom', as worthy of the Physics Nobel Prize.<sup>7</sup> The Committee claimed that Bohr's hypotheses of 1913 that the atom can exist in different states, each of which is characterized by a given energy level, and that these energy levels govern the spectral lines emitted by the atoms, were no longer mere hypotheses but experimentally proved facts. 'The methods of verifying these hypotheses,' the Committee continued in its citation, 'are the work of James Franck and Gustav Hertz.'<sup>8</sup>

In fairness to Franck and Hertz, it should be noted that they admitted their error of interpretation in their Nobel lectures. 'It appeared to me to be completely incomprehensible,' Franck observed in his lecture,

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1. Townsend and Ayres, 1924, p.401. 5. Ibid.

2. Ibid., p.403. 6. Ibid.

3. Ibid., p.415. 7. Nobel Lectures, 1965, p.93.

4. Ibid., p.414 (my emphases). 8. Ibid., p.96.

'that we had failed to recognize the fundamental significance of Bohr's theory, so much so, that we never even mentioned it once in the relevant paper. It was unfortunate that we could not rectify our error... ourselves by clearing up the still existing uncertainties experimentally. The proof that... the gas is not simultaneously ionized', Franck continued to observe, 'came about... during the war, period through suggestions from Bohr himself and from Van der Bijl.'<sup>1</sup>

Hertz, on his part, stated explicitly that at the time they 'erroneously believed that... [what they had measured] was the ionization potential'.<sup>2</sup> He went on to conclude that 'all the results so far attained with the electron-impact method agree very closely with Bohr's theory'.<sup>3</sup> Having realized the importance of Bohr's theory, Franck ended his lecture thus: 'We know only too well that we owe the wide recognition that our work has received to contact with the great concepts and ideas of M. Planck and particularly of N. Bohr.'<sup>4</sup>

In principle, the experiment of Franck and Hertz created the possibility of discovering experimentally the existence of atomic energy levels. However, in view of their error of interpretation, the recognition that an atom is capable of assuming different energy states and can undergo - through a process of radiation - a transition to a lower energy state, must be credited to Bohr and not to Franck and Hertz.

In order to justify his position, Townsend had to pursue an extensive and critical study of the Franck and Hertz experiment with a view to finding errors in its underlying theory and the set-up involved. The principal issue of this study was the process of transference of energy in collisions between electrons and the gas molecules. In particular, Townsend questioned the validity of the following hypotheses which, according to him, comprised the core of the theoretical consideration of Franck and Hertz:

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1. Ibid., p.106.
  2. Hertz, 1965, p.124.
  3. Ibid., p.128.
  4. Ibid., p.108.

'Electrons moving with small velocities rebound without any loss of energy whatever from atoms with which they collide.

As the velocity of the electron is increased a point is reached where the electron loses all its energy in a collision.

The only amounts of energy which an electron may lose in a collision are the precise amounts corresponding to resonance potentials or the ionizing potential.'<sup>1</sup>

These hypotheses were at variance with the results Townsend had obtained from his experiments. He argued that the hypotheses 'are not in agreement with the principle of the conservation of momentum', and that 'there are no experiments which show that the loss of energy is absolutely negligible in collisions where the direction of motion of the electrons is changed'; but 'there are several experiments which show that electrons lose only a small proportion of their energy in collisions with molecules'.<sup>2</sup>

Concerning possible errors in the Franck and Hertz experimental procedure. Townsend pointed out that 'in estimating the correction for contact potential and the distribution of the velocities of the electrons an error amounting to 0.7 volt may occur in the absolute values of the resonance potentials'.<sup>3</sup> Another source of error, Townsend argued, was the effect of space charge. He maintained that 'a considerable error may... be made in treating the space inside a metal enclosure as a "force free" space';<sup>4</sup> as the space charge constitutes a retarding field with respect to the electrons, it is desirable, he suggested, to take its force into consideration.<sup>5</sup> However, with respect to the photoelectric effect - an effect whose relevance to this study Bohr and Van der Bijl had stressed - Townsend was of the opinion that it 'must be extremely small in comparison with the effect of ionization by collision'.<sup>6</sup>

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1. Townsend and Focken, 1926, p.475.      4. Ibid., p.477.

2. Ibid.

5. Ibid.

3. Ibid., p.476.

6. Ibid., p.478.



Having argued against the conclusions of the Franck and Hertz experiment, Townsend reiterated his position claiming that

the motion of electrons through a gas is completely controlled by the losses of energy in small amounts when the electric forces are small, so that when the forces are increased and ionization by collision begins to appear the losses in the smaller amounts cannot be left out of consideration.<sup>1</sup>

Townsend concluded therefore that 'the theory of the transfer of energy in quanta as it is generally stated is found to be quite untenable'.<sup>2</sup>

Since the quantum theory was at that time gaining prominence as the principal theory for any discussion concerning the phenomenon of energy-interchanges between atoms, molecules and free electrons, an explanation of Townsend's findings was much required. This task was taken up by Atkinson who, in 1928, attempted to reconcile 'the experiments of the one party [namely, Townsend's] with the theories of the other [namely, Franck's and Hertz's]'.<sup>3</sup>

At the outset, Atkinson observed that 'if the Quantum Theory statements are broadly correct,... [then Townsend's experiments] are of a type unsuited to the problem'.<sup>4</sup> Atkinson attempted, therefore, to establish that the Townsend experiments were not fitted for the purpose of tackling the problem of energy-interchanges between free electrons and gas molecules. Pursuing this study, Atkinson realized the methodological error involved in Townsend's experiments. 'The statistical interpretation of these experiments,' Atkinson argued, is 'open to distrust on its own merits alone.'<sup>5</sup> Furthermore, even if an alternative statistical interpretation was admissible, it would have been in any case 'unsuited to give any estimate of the critical potential'.<sup>6</sup> Townsend's experiments were simply unsuitable for accurate quantitative study of ionization by collisions.

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1. Ibid., p.484.

2. Ibid., p.474.

3. Atkinson, 1928,a, p.335.

4. Ibid.

5. Ibid., p.345.

6. Ibid.

Atkinson pursued his objections to Townsend's results along three lines. He pointed out the enormous effect of the metastable states when statistical methods are employed (an effect which was quite unsuspected);<sup>1</sup> he criticized the use Townsend had made of statistical concepts like the 'average loss of energy on collision'<sup>2</sup> and the 'distribution of velocities';<sup>3</sup> and thirdly, he noted an apparent misapprehension of the quantum theory on the part of Townsend.<sup>4</sup>

In another paper, Atkinson observed that Townsend had treated the problem of ionization by collision 'in terms of the average energy possessed by all the electrons at any given plane, the average energy lost in a "cycle" by an electron, the average energy at which ionizations occur, and so on'.<sup>5</sup> Atkinson argued therefore that all that Townsend's 'measurements suggest is that, when the average energy of all the electrons is large, the average losses are also large enough to indicate a fair degree of average inelasticity'.<sup>6</sup> The phenomenon of a loss of energy of definite discrete value at an inelastic collision - an effect which can be confirmed by optical data - simply has no place in experiments of such a statistical nature. To be sure, Townsend's experiments appear very well suited to account qualitatively for the existence of an exponential curve for a limited range of potentials; but then, as Atkinson remarked,

it is evident that the exact figure for the exponential constant obtained is almost valueless, since it depends on so many unknown interacting factors, such as the nature and relative proportions of the various impurities present, the amount (if any) of energy which

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1. Ibid., pp.339-43.

2. Ibid., pp.343-44.

3. Ibid., pp.344-45.

4. Ibid., pp.346-47.

5. Atkinson, 1928,b, pp.440-41 (emphases in the original).

6. Ibid., p.441.

the newly formed electron may possess, the relative effective diameters of the molecules in question,

and some other factors.<sup>1</sup>

We may conclude with Loeb that

the Franck and Hertz experiments and the evaluation of the ionization and excitation potentials by them succeeded because they worked at low pressures and studied the results of individual impacts at the requisite energy. Townsend's experiments, although correct and of significance, failed to be amenable to interpretation and to a simple evaluation of the ionizing potential for the reason that at the higher pressures the energy gain owing to elastic impacts and the probabilities of excitation and ionization loss make the evaluation of the electron energy impossible.<sup>2</sup>

In view of the proposed classification of experimental error, we consider Franck's and Hertz's error an error of the fourth kind, whereas the error of Townsend is of the first kind.

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1. Atkinson, 1928,a, p.342.

2. Loeb, 1939, p.361.

## CHAPTER VI

### Kaufmann's Experiment and Its Reception

'The [experimental] results speak decisively against the correctness of Lorentz's theory and consequently also of that of Einstein's theory. If one were to consider these theories as thereby refuted, then the attempt to base the whole of physics, including electrodynamics and optics, upon the principle of relative motion would have to be regarded at present also unsuccessful.'<sup>1</sup>

W. Kaufmann

'Kaufmann's conclusion... that... electrons have no material mass at all... is certainly one of the most important results of modern physics.'<sup>2</sup>

H.A. Lorentz

'As a matter of fact, the velocity dependence of energy and of mass has nothing... to do with the structure of the body considered, but is a general relativistic effect. Before this became clear, many theoreticians wrote voluminous, not to say monstrous, papers on the electromagnetic self-energy of the rigid electron... Today all these efforts appear rather wasted; quantum theory has shifted the point of view, and at present the tendency is to circumvent the problem of self-energy rather than to solve it. But one day it will return to the centre of the scene.'<sup>3</sup>

M. Born

'There was a time when dynamic rather than kinematic arguments led to the notion of electromagnetic mass, a form of energy arising specifically in the case of a charged particle coupled to its own electromagnetic field... The investigations of the self-energy problem of the electron by men like Abraham, Lorentz, and Poincaré have long since ceased to be relevant. All that has remained from those early times is that we still do not understand the problem... We still do not know what causes the electron to weigh.'<sup>4</sup>

A. Pais

A statement may be recognized as erroneous only when it is embedded within a set of ideas which claims to constitute knowledge. The possibility of equating sin with error in the realm of religion arises from the belief that a true conduct of religious life can be known and attained. Likewise,

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1. Kaufmann, 1906,a, p.534.

2. Lorentz, 1952, p.43.

3. Born, 1956, p.196.

4. Pais, 1983, pp.155, 159.

in science an experimental error is, so to speak, committed when a statement about the physical world - a statement which has been arrived at through experimentation - does not in fact conform to the reality one believes one knows. A claim to knowledge may be construed as an error when it is re-examined in the light of a set of ideas or experimental results which present reality differently. Error, whether it is committed in the religious or in the scientific realm, is a failure of an epistemic system or an element of it. To apprehend such a failure one needs a vantage point from which what was thought to be a true conduct or a correct statement is found to be erroneous. In the religious realm this vantage point is dogmatically given, but in science the scientist must ultimately seek it by himself.<sup>1</sup>

I therefore claim that an experimental error of a particular experiment may exhibit different features when observed from different points of view, that is, different philosophical outlooks. In other words, the identification of an experimental error may depend on the philosophical perspective from which the experiment is viewed critically. My intention is not to elaborate a certain relativism of the concept of error, but rather to underline the connection between philosophical outlook and detection of experimental error.

A case in point is Kaufmann's experiment whose results were viewed at the beginning of the century from several theoretical viewpoints; consequently, the error in this set of experiments assumed at that time different characteristics: from not being an error at all, through an error of interpretation and insufficient accuracy to a failure in the apparatus.<sup>2</sup>

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1. To be sure, science is a communal activity and one often has to accept results on human authority; but ultimately one has to exercise one's own judgement. As Wittgenstein puts it, 'I learned an enormous amount and accepted it on human authority, and then I found some things confirmed or disconfirmed by my own experience'. (Wittgenstein, 1977, p.23e (§161).) See also *supra*, p.151 footnote no.1.
  2. Two works which rectify the distorted historical treatment this episode has received in physics textbooks, have been recently published: Cushing, 1981, and Miller, 1981. In the course of drawing the historical scene of this set of experiments I frequently refer to these works. The historical scene provides the background against which I detail the responses of Poincaré, Einstein and Lorentz which were expressed from different viewpoints. Reference should also be made to Zahar, 1978. Zahar discusses the logical and philosophical implications of Planck's response to Kaufmann's experiments. For an overview of the historical development of the theory of the electron see Pais, 1972.

### 6.1 The experiment

The question whether or not mass may modify its amount in the course of its motion was one of the problems dealt with in Stokes' researches in hydrodynamics. In 1842 Stokes analysed the slow motion of a solid, perfectly smooth sphere, through an incompressible perfect fluid, and reached the conclusion that the motion of such a body can be described as if it had a modified mass. Clearly, a sphere of mass  $m_0$  which requires an energy of  $(1/2)m_0 v^2$  to reach a velocity  $v$  in a vacuum, would require a greater amount of energy, to reach the same velocity, when it is immersed in a fluid; for the sphere, when it is in motion, will set the fluid around it in motion. Thus, the source of energy which drives the sphere to a velocity  $v$ , will have to supply additional energy to the fluid. Stokes argued that the true mass of the sphere,  $m_0$ , can be viewed as if it was modified while in motion to  $m_0 + m'$ , where  $m'$  is a constant depending on the radius of the sphere and on the density of the fluid.<sup>1</sup>

If one were to suppose that the forces emanating from a moving charged corpuscle could set the ether in motion and assume further that the ether has some kind of mass, then the theoretical set-up of this case would be analogous to the case of the moving sphere in a fluid. Consequently, one would expect an increase in the mass of the charged corpuscle while it is in motion. It thus comes as no surprise that as early as 1881, on completely theoretical ground and well before his experiments on the existence and properties of cathode-ray corpuscles, J.J. Thomson could introduce the concept of electromagnetic mass.<sup>2</sup>

In response to the new researches of Crooks and Goldstein in 1880 on electric discharges in high vacuum,<sup>3</sup> J.J. Thomson set himself the task,

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1. Stokes, 1966, p.17 (my emphases). Cf., J.J. Thomson, 1907, pp.29,30; Lorentz, 1931, p.256.
  2. J.J. Thomson, 1881, pp.230, 234. Cf., J.J. Thomson, 1907, pp.29-30. It should be borne in mind that this concept is based on a dynamic argument.
  3. For references see J.J. Thomson, 1881, p.229.

as he formulated it, 'to take some theory of electrical action and find what, according to it, is the force existing between two moving electrified bodies, what is the magnetic force produced by such a moving body, and in what way the body is affected by a magnet'.<sup>1</sup> On the basis of Maxwell's theory, J.J. Thomson pointed out that a moving charged particle is surrounded by a magnetic field which possesses energy, hence more work will have to be done to start or stop the particle than if it were uncharged.

Thus, with no novel departure - assuming that the surface charge distribution on the moving particle remains unaltered by the motion and that the electric field is carried forward undistorted with the moving particle<sup>2</sup> - J.J. Thomson calculated that the 'whole kinetic energy' of the moving charged corpuscle, which he for simplicity considered a sphere, is  $T = (m/2 + (2/15)\mu e^2/a)v^2$ , where  $\mu$  is the coefficient of the magnetic permeability of the medium through which a sphere of radius  $a$  and charge  $e$  travels with a velocity  $v$ . Hence his conclusion that 'the effect of the electrification is the same as if the mass of the sphere were increased by  $(4/15)\mu e^2/a$ '.<sup>3</sup>

In 1889 O. Heaviside corrected the numerical coefficient to  $2/3$  for velocities which are very small compared to that of light, and observed that 'as the speed increases, the electromagnetic field concentrates itself more and more about the equatorial plane  $\theta = \pi/2$ '.<sup>4</sup> Heaviside made then the important point that in the limiting case  $v=c$ ,  $E$  and  $H$  become zero everywhere except in the equatorial plane; that is, the electromagnetic field becomes in this limiting case a plane electromagnetic wave.<sup>5</sup>

J.J. Thomson took up this point and elaborated it in his Notes on Recent

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1. Ibid.

2. Jammer, 1961, p.136.

3. J.J. Thomson, 1881, p.234 (my emphases). Lodge described this paper as 'epoch-making'. (Lodge, 1907, p.17.)

4. Heaviside, 1889, p.332.

5. Ibid.

Researches in Electricity and Magnetism; he stated quite specifically that in the limit  $v=c$ , 'the increase in mass is infinite, thus a charged sphere moving with the velocity of light behaves as if its mass were infinite, its velocity therefore will remain constant, in other words', J.J. Thomson inferred, 'it is impossible to increase the velocity of a charged body moving through the dielectric beyond that of light'.<sup>1</sup>

Since the electromagnetic mass increases with the velocity of the charged body up to the limiting case of the velocity of light, it became apparent that there might be experimental means of separating the charged body's effective mass into its 'true' (mechanical) and 'induced' (electromagnetic) parts.<sup>2</sup>

It appears therefore that the qualitative behaviour of a moving charged corpuscle, including its bounded velocity, had been secured fairly early before any experiment concerning this investigation was carried out.

Naturally, J.J. Thomson brought these theoretical considerations to his research on the nature and properties of cathode rays. Having measured the velocity of cathode rays - which he found to be very small in comparison to that of light<sup>3</sup> - he probably did not expect the mass of the claimed constituent charged corpuscles, and therefore the ratio  $e/m$ , to be affected greatly by their velocity. However, he was cautious not to commit himself to the view that the ratio  $e/m$  is independent of velocity. Kaufmann's experimental results at the turn of the century of the ratio  $e/m$  for high

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1. J.J. Thomson, 1893, p.21 (my emphases).

2. See, for example, Lorentz, 1952, p.40. Cf., Lorentz, 1931, p.256.

3. Seeking to discriminate between the two views held as to the nature of cathode rays (see supra, pp.210-11), J.J. Thomson determined the velocity of these rays. He argued that 'if we take the view that the cathode-rays are aetherial waves, we should expect them to travel with a velocity comparable with that of light; while if the rays consist of molecular streams, the velocity of these rays will be the velocity of the molecules, which we should expect to be very much smaller than that of light'. His measurements yielded a velocity of  $1.9 \times 10^7$  cm./sec. (J.J. Thomson, 1894, pp.360, 364.)



speed negatively charged corpuscles, that is, electrons, were therefore qualitatively expected and came as no surprise. At stake was rather a quantitative theory which required definite assumptions about the structure of the electron and the distribution of the charge on it - in a word, a theory of the electron.

Kaufmann's research into the nature of the electron can be divided chronologically<sup>1</sup> to two distinct periods. From 1898 to 1903 - in the wake of J.J. Thomson's 1897 experimental results concerning the constitution of cathode rays - one finds Kaufmann experimenting with the deflections of moving electrons in electric and magnetic fields. Using  $\beta$ -rays, or Becquerel rays as they were called at the time, Kaufmann reached in 1901 some qualitative conclusions with regard to the mass of fast-moving electrons. At the end of this period, in 1902-1903, Kaufmann surpassed the qualitative nature of his experiments by increasing the accuracy and adopting a specific theory of electrons, namely, that of M. Abraham which assumes an undeformable, rigid, spherical electron; having adopted it he proceeded to corroborate it.

The second period of Kaufmann's research starts in 1905 with his invention of a rotating mercury air pump, which he put into practice in his experiments of 1905 and 1906. These experiments were essentially similar to the early ones but, in the face of mounting criticism, designed with greatly increased accuracy to discriminate between several theories of electron: the theories of Abraham, Bucherer, Lorentz, and that of Einstein which formally agrees with that of Lorentz. Kaufmann's judgement was that whatever the correct theory is, it is definitely not that of Lorentz and Einstein. Kaufmann defended his conclusion from thenceforth in spite of the highly critical views which his results had received.<sup>1</sup> By 1915 more experimental results from other types of experiment became available; the

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1. 'As far as I know after 1908 Kaufmann ceased to publish on  $\beta$ -ray deflection experiments.' (Miller, 1981, p.377 note no.10.)

consensus was that Kaufmann's experiments were in error and that the Abraham classical picture of the rigid electron had to be ruled out.<sup>1</sup>

However, while analysing in 1938 the classical experiments on the relativistic variation of electron mass, C.T. Zahn and A.H. Spees reached the conclusion that for the higher velocities 'no very satisfactory experimental distinction between the two types of electron has as yet been made by direct electric and magnetic deflection methods'.<sup>2</sup> And they remarked that 'in view of the fundamental importance of such experiments it seems that much is left to be desired'.<sup>3</sup>

In 1957, almost twenty years later, the situation seemed not to have changed much. In their review of the experimental evidence for the law of variation of the electron mass with velocity, P.S. Faragó and L. Janossy concluded that 'it is the fine-structure splitting in the spectra of atoms of the hydrogen type which give the only high-precision confirmation of the relativistic law of the variation of electron mass with velocity'.<sup>4</sup> They however remarked that 'this evidence... is a rather indirect one, and it does not cover a range of velocities which is wide enough'.<sup>5</sup> Concerning the fairly large number of direct experiments on the behaviour of free electrons, these authors maintained that they 'could hardly find such results which would prove the validity of the relativistic relation with a margin of error much less than, say, the difference between the results of the relativity theory and the theory of Abraham'.<sup>6</sup> This critical view of Faragó and Janossy points at the heart of the problem: to distinguish experimentally between Abraham's theory of electrons and that of Lorentz

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1. Lorentz, 1931, pp.274-88; Miller, *ibid.*, pp.345-52.

2. Zahn and Spees, 1938, p.511.

3. *Ibid.* Cf., Miller, 1981, pp.351-52.

4. Faragó and Janossy, 1957, p.1436.

5. *Ibid.*

6. *Ibid.*

and, for that matter, Einstein's theory, a margin of experimental error which is less than the difference between the respective theoretical results has to be attained. As we shall see, this is a considerable task since the difference is indeed very small.<sup>1</sup>

The origin of the experimental method which W. Kaufmann (1871-1947) employed in his research into the nature of the electromagnetic mass goes back to the work of H. Hertz who, as early as 1883, tried - though unsuccessfully - to detect a deflection of cathode rays when they were made to pass through an electric and magnetic field.<sup>2</sup> This method of research was perfected in 1897 by J.J. Thomson who - having evacuated his cathode-ray tube to a higher degree of exhaustion than that of Hertz - succeeded in obtaining and indeed measuring such deflections.<sup>3</sup> According to J.J. Thomson his experimental results confirmed the view that a stream of identical corpuscles would behave exactly like cathode rays if the corpuscles were charged with electricity and projected from the cathode by an electric field. He explained that these corpuscles 'would evidently give a value of  $m/e$  [sic] which is independent of the nature of the gas and its pressure, for the carriers are the same whatever the gas may be'.<sup>4</sup>

Kaufmann himself was not far from discovering these corpuscles. Conducting cathode-ray experiments he concluded that

if one makes the plausible assumption that the moving particles are ions, then  $e/m$  should have a different value for each substance and the deflection [in electric and magnetic fields] should depend on the nature of the electrodes or on the nature of the gas [in the cathode tube]. Neither is the case. Moreover, a simple calculation shows that the explanation of the observed deflections demands that  $e/m$  should be about  $10^7$ , while even for hydrogen [ $e/m$ ] is only about  $10^4$ .<sup>5</sup>

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1. For the difference between the respective theories see Abraham, 1904, p.578; Jammer, 1961; Goldberg, 1970-1971,a,p.18.
  2. Supra, case-study 5.2.
  3. J.J. Thomson, 1897,b.
  4. Ibid., p.311.
  5. Kaufmann, 1897, p.545. Quoted by Pais, 1983, p.156.

Pais speculated that 'had Kaufmann added one conjectural sentence to his paper, completed in April 1887, he would have been remembered as an independent discoverer of the electron'.<sup>1</sup> At the end of that month, J.J. Thomson informed the Royal Institution that his experimental results 'favour the hypothesis that the carriers of the charges are smaller than the atoms of hydrogen'.<sup>2</sup> In Pais' view, 'Kaufmann's paper deserves to be remembered even though he lacked Thomson's audacity in making the final jump towards the physics of new particles'.<sup>3</sup> However, concluding his paper Kaufmann remarked that 'the hypothesis accepting that cathode rays are emitted particles is not sufficient for a satisfactory clarification of the relations observed by me'.<sup>4</sup>

In this climate of new discoveries one finds Kaufmann attempting in 1898 to establish experimentally a correlation between the magnetic deflection of cathode rays and the accelerating electric potential.<sup>5</sup> Pursuing this research he began to realize the importance of the ratio  $e/m$  to this study and therefore focused his attention upon it. He decided to repeat, with the greatest attainable accuracy, those experiments which bore upon the determination of  $e/m$ .<sup>6</sup>

Kaufmann set his student, S. Simon, to work on this problem. By using some of Kaufmann's techniques, e.g. the transmission of a beam of cathode rays through a magnetizing coil, Simon attempted to obtain as accurate

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1. Pais, *ibid.*

2. J.J. Thomson, 1897,a, p.109.

3. Pais, *op.cit.*

4. Kaufmann, 1897, p.552. Quoted by Miller, 1981, p.107.

5. Kaufmann, 1898,a; cf., *Sci.Abs.*, *Phys. Elec. Eng.*, 1(1898), p.549 art.1049. Kaufmann, 1898,b; cf., *Sci.Abs.*, 2(1899), p.114 art. 282. Cf., Miller, *ibid.*, pp.107-8.

6. Kaufmann, *ibid.*

a value as possible for this ratio.<sup>1</sup> He recognized that knowledge of the variation of the magnetic field along the path of the cathode-ray beam is central to this measurement. He therefore designed a special magnetometer and was particularly careful to obtain as uniform a field as possible. Simon's final value for the ratio  $e/m$  for cathode rays was  $1.865 \times 10^7$  c.g.s. units; a value to which Kaufmann referred approvingly and upon which he indeed relied in his own researches.<sup>2</sup> Since the accepted value of  $e/m$  for cathode rays, or  $e/m_0$ , is  $1.759 \times 10^7$ , Kaufmann's reliance on Simon's result was at fault; however, as we shall see, Kaufmann was aware of this possibility.<sup>3</sup>

Notwithstanding the importance of these experimental investigations, the attention of the scientific community was directed at that time to the discoveries of new forms of radiation. Experimenting with these, apparently spontaneous, forms of radiation, Becquerel, Giesel, Meyer and Schweidler demonstrated that Becquerel rays - a form of radiation which Rutherford had called  $\beta$ -rays<sup>4</sup> - were deflected by a magnetic field. Subsequent studies by the Curies showed that these rays carry negative electric charge; and when in 1900 Becquerel succeeded in deviating them by an electric field, he was giving substance to the suggestion that  $\beta$ -rays are of the same nature as cathode rays with the important difference that the former are much swifter than the latter.<sup>5</sup>

Kaufmann was quick to capitalize on this important and significant discovery; it simply gave him the tool he was seeking: swift particles

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1. S. Simon, 1899; cf., Sci.Abs., 3(1900), p.42 art.118.

2. E.g., in his definitive paper, 1906,a, p.533.

3. The more accurate value is  $(1.75890 \pm 0.00002) \times 10^7 \text{ emug}^{-1}$ . (See, E.R. Cohen, K.M. Crowe and J.W.M. DuMond, 1957, p.267.) On the usage of the two systems, esu and emu, see Miller, 1981, pp.45-46.

4. Rutherford, 1899.

5. See Whittaker, 1953, p.3.

which approach the velocity of light. He abandoned therefore his experiments on cathode rays and concentrated on new investigations on the velocity of Becquerel rays and the ratio of the electric charge of the carriers to their mass. In his preliminary description of these investigations in 1901 he outlined a photographic method by which the deflections due to magnetic and electric fields could be measured. Kaufmann was at pains to stress that with his design he had attempted to overcome difficulties which might arise from the fact that the velocity spectrum of the  $\beta$ -rays was 'inhomogeneous' - the constituent particles having different velocities - and, furthermore, from the possibility of attaining too poor a vacuum in which case the rays might have made the surrounding gas a conductor.<sup>1</sup> These cautious considerations may explain why Kaufmann was considered a trustworthy experimenter.

To overcome the problem of distribution of velocities of the  $\beta$ -rays, Kaufmann borrowed a technique from optics: Kundt's method of crossed spectra. Seeking to demonstrate the phenomenon of anomalous dispersion, Kundt - in a series of experiments performed in 1871-1872 - let a collimated beam of sun light pass through two prisms whose refractive faces were oriented normally; hence the name crossed spectra. In his electromagnetic analogue of Kundt's optical method, Kaufmann let a collimated beam of  $\beta$ -rays pass normally through parallel electric and magnetic fields which spread out the inhomogeneous  $\beta$ -ray beam into a curve on the photographic plate. Whereas with J.J. Thomson's technique one obtains a spot on the fluorescent screen by letting the beam of radiation pass normally to a combined field of electric and magnetic fields which are oriented normally to each other, with Kaufmann's technique one obtains a curve as the parallel electric and magnetic fields spread out the beam.<sup>2</sup>

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1. Kaufmann, 1901, a; cf., Sci.Abs., 4(1901), p.934 art.2111. See also Miller, 1981, p.48.

2. Miller pointed out that by opting for an analogue of Kundt's method over J.J. Thomson's crossed-field arrangement, Kaufmann had made a tactical error because analysis of the resulting complicated curve was fraught with too many possibilities for error'. (Miller, 1981, p.51, see also pp.49-50.)

Following up these investigations, Kaufmann published in 1901 his first major paper in this field, 'The Deflection of Becquerel Rays and the Apparent Mass of the Electrons'.<sup>1</sup> In this paper he set himself the problem 'to determine the speed as well as the ratio  $e/m$  as accurately as possible for Becquerel rays and... from the degree of dependence of  $e/m$  on  $v$  to determine the relation between "actual" [wirklich] and "apparent" [scheinbar] mass'.<sup>2</sup> Since he assumed that Becquerel rays are swift cathode rays, he began his paper with some arguments in support of this view. He observed, for example, that 'experiments on cathode rays have shown that with increased speed the deflectability decreases and the penetrability increases',<sup>3</sup> which is entirely in keeping with the phenomena that Becquerel rays exhibit: the magnetic deflection of Becquerel rays is much smaller and their ability to penetrate solids much greater than that of cathode rays.

Having established that Becquerel rays are in fact swift cathode rays, Kaufmann delineated the method he had employed. The idea was to secure with a diaphragm a pencil of Becquerel rays and to record its point image on a photographic plate placed perpendicularly to the beam; a simultaneous application of an electric field and, parallel to it, a magnetic field would change the point image into a curve as the deflection due to the latter field is in a direction at right angles to that due to the former field. Each point of the resulting curve corresponds to definite values of  $v$  and  $e/m$ . A whole series of observations is thus obtained on a single photographic plate from which, as Kaufmann remarked, 'the dependence of  $e/m$  on  $v$  can be read off directly'.<sup>4</sup>

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1. Kaufmann, 1901,b; cf., Sci.Abs., 5(1902), p.296 art.685. For an English translation see Boorse and Motz, 1966, p.506.
  2. Boorse and Motz, *ibid.*, p.507.
  3. *Ibid.*, p.506.
  4. *Ibid.*, p.507.

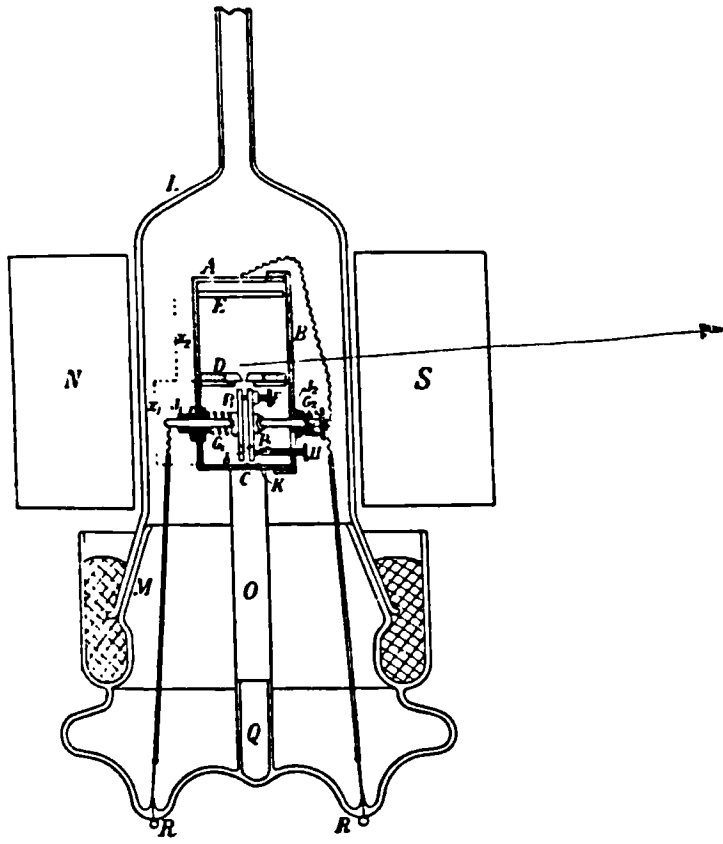


Fig. 1a

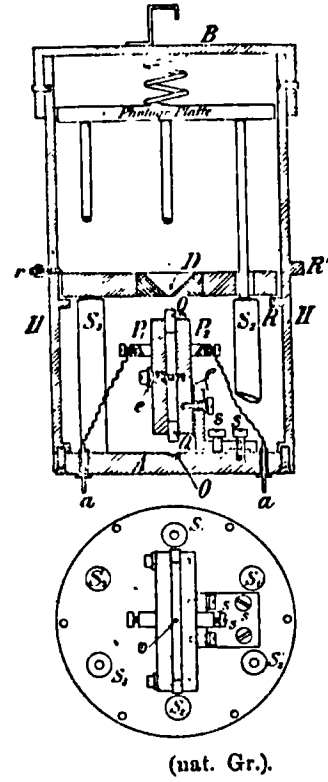


Fig. 1b

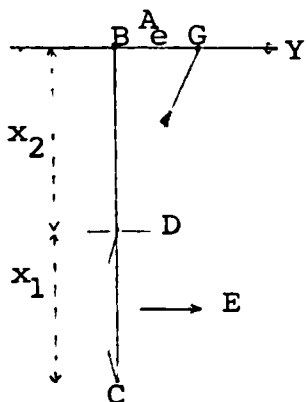


Fig. 2

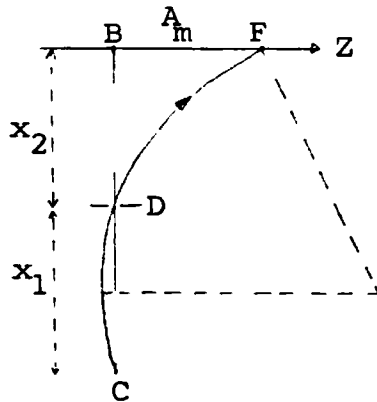


Fig. 3



Fig. 5

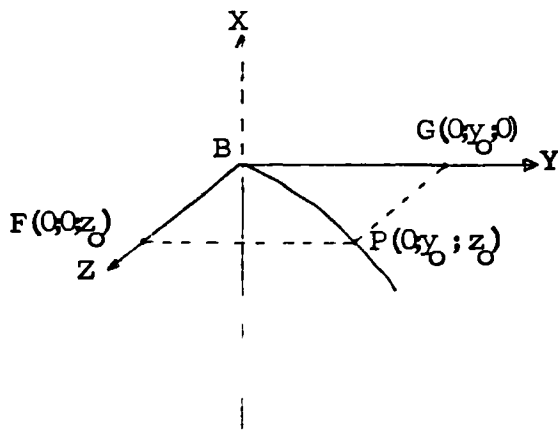


Fig. 4



To obtain these observations Kaufmann placed a speck of Radium Bromide at C in a glass vessel from which the air was extracted (Fig.1a). He secured a fine pencil of  $\beta$ -rays by letting the particles pass through a small aperture, D, about half a millimetre in diameter, in a lead diaphragm. He collected the rays on a photographic plate, E, placed about 2cm. from the diaphragm. Kaufmann subjected the particles in their passage from C to D - a length of about 2cm. - to an electric field maintained at a potential difference of several thousands volts across the parallel plates  $P_1P_2$  which were about 1.5 mm. apart. He introduced the evacuated glass vessel between the poles of a stack of permanent magnets NS, so that during the whole of their flight from C to E the particles were subjected to a magnetic field of the order of hundreds of Gauss which was oriented, as required, parallel to the electric field. The undeflected  $\gamma$ -rays marked on the photographic plate the direct 'line of fire'; its point image, B, acted as the geometrical origin for the resulting curve (Fig.4).<sup>1</sup>

This arrangement of the fields and the diaphragm acted as a kind of velocity filter. Since the particles were deflected in their passage from C to D, only those which were to have the appropriate velocities for passing through the aperture in the diaphragm could reach the photographic plate; all the others struck the bounding surfaces of either the condenser or the diaphragm and did not reach the plate. Kaufmann thus obtained a well defined curve.

The theory which underlies Kaufmann's experiment is that which governs the electric and the magnetic deflections of moving charged particles within the confinement of the geometrical arrangement of the apparatus. In this case the trajectory of a charged particle which is subject only to an electric field E is parabolic between C and D, and rectilinear in the rest of its flight in the x-y plane (Fig.2). When the particle is

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1. Ibid. Cf., J.J. Thomson, 1906, pp.653-55; Lodge, 1907, pp.138-45; Miller, 1981, pp.48-54; Cushing, 1981, pp.1138ff.

subject only to a magnetic field  $H$ , its trajectory describes a circle CDF in the  $x$ - $z$  plane, perpendicular to the direction of the field (Fig.3).

Clearly, if all the particles were to have the same velocity they would have produced on the photographic plate a single dot; but as they have different velocities one obtains, under the combined fields, a series of dots which jointly form a curved line in the  $y$ - $z$  plane. One can then measure with a micrometer the coordinates  $(y_0, z_0)$  of a point  $P$  on the curve and determine thereby the electric deflection,  $A_e$ , and the magnetic deflection,  $A_m$ , respectively (Fig.4).

The calculation of the ratio between the two deflections shows that  $A_m/A_e = (1/k_1)v$ , where  $k_1$  is a constant depending only on the relative effective strengths of the fields. From the equations for the respective deflections one obtains for the ratio  $e/m = kA_m^2/A_e$ , where  $k$  is another constant which is expressed in terms of the experimental set-up.

As it was suggested that mass varies with velocity, one considers further a velocity function  $\bar{\Phi}(\beta) = m/m_0$ , where  $m_0$  is the mass of slowly moving charged particles like cathode rays and  $\beta$  is  $v/c$ ; hence,  $\bar{\Phi}(k_1 A_m/c A_e) = m/m_0$ . The equation for the measured deflections obtained on the photographic plate is therefore  $(A_m^2/A_e) \bar{\Phi}(k_1 A_m/c A_e) = k_2$ , where  $k_2$  is another constant.

The experiment thus consists in getting on the photographic plate as clear a curve as possible, and in measuring  $A_e$  and  $A_m$  for any particular point on the curve. If, by adjusting  $E$  and  $H$ , a constant  $k_1$  can be obtained so that the calculated value of  $k_2$  for a given curve will remain constant within the limits of experimental error, then the form of the hypothetical function  $\bar{\Phi}(\beta)$  is to that extent verified; consequently, the underlying assumed structure of the particle and its charge distribution are thereby to that extent verified as well.<sup>1</sup>

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1. Kaufmann, 1901,b. This is a general outline of the experimental set-up and its theoretical underpinning; for a detailed and in-depth analysis, see Lodge, 1907, pp.140-42; Lorentz, 1931, pp.272-74; Miller, 1981, pp.48-54, 61-67; Cushing, 1981.

Kaufmann's set-up remained essentially the same throughout his researches; however, he did change the method of reducing the data.<sup>1</sup> To increase the accuracy he made some of the dimensions slightly smaller and used a more active Radium Bromide supplied by the Curies. As a consequence, he could apply fields of lesser strength which facilitated the maintenance of their required homogeneity.

Fig.1b is a full-scale diagram of the core of Kaufmann's apparatus as it appears in his definitive paper on this subject.<sup>2</sup> Some 48 hours of exposure were needed to produce a perceptible curve suitable for analysis. Fig.5 is the actual size of Kaufmann's best run of the 1902-1903 series of experiments.<sup>3</sup> By reversing the electric field Kaufmann obtained two curves; the symmetry of the curves gave an indication of the uniformity of the field. Such a photographic plate made G.N. Lewis caution physicists in 1908 not to forget that Kaufmann's measurements - though taken with extreme care and delicacy - 'consisted in the determination of the minute displacement of a somewhat hazy spot on a photographic plate'.<sup>4</sup>

Kaufmann was the first experimenter to establish experimentally that, as he put it, 'with increasing  $v$  the ratio  $e/m$  decreases very markedly, from which', he argued, 'one may infer the presence of a not inconsiderable fraction of "apparent mass" which increases with speed in such a way as to become infinite at the speed of light'.<sup>5</sup> Using Searle's formula for the field energy of a rapidly moving electron,<sup>6</sup> Kaufmann concluded that 'the ratio of apparent to true mass for speeds that are small with respect

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1. Miller, *ibid.*, pp.51-54 (1901 data), 61-67 (1902-1903 data), 228-32 (1905-1906 data); Cushing, *ibid.*, pp.1138-41 (1901-1903 data), 1142 (1905-1906 data).

2. Kaufmann, 1906,a, p.496.

3. See Miller, 1981, p.65. Kaufmann, 1903, p.401.

4. Lewis, 1908, p.713.

5. Boorse and Motz, 1966, p.509.

6. Searle, 1897. Searle considered the electron an infinitely thin, charged, spherical shell. See also Jammer, 1961, p.140; cf., Cushing, 1981, pp.1140-41.

to the speed of light is... about  $1/3$ '.<sup>1</sup> Thus, the so-called true mass had to be considered, at least for this range of velocities, three times greater than the "apparent" mass.

Appropriately, the paper ends with a cautious remark: having employed Searle's formula Kaufmann found himself obliged to point out that his conclusion

depends on the assumption that the charge of the electron is distributed over an infinitely thin spherical shell. Since we know nothing about the constitution of the electron and we are not justified a priori in applying to the electron the laws of electrostatic which we seek to derive from the properties of the electron itself, it is quite possible that the energy relationships of the electron can be derived from other charge distributions, and that there may be distributions which, when applied to the above analysis, give a zero true mass.<sup>2</sup>

It is worth noting that here Kaufmann made explicit the possible circularity of the argument. The claim is that if one were to endow the electron a priori with some definite structure and proceed to analyse on this basis the physical properties which the electron exhibits, one might not be then entitled to consider one's results conclusive: the theoretical basis being presupposed and proved at one and the same time. In any event, on the basis of some other structure the analysis would arrive at different results. Kaufmann found himself therefore free to envisage a different structure which would render the true mass zero.

Kaufmann estimated that the accuracy of his 1901 results were about 5%.<sup>3</sup> He obtained a value of  $1.95 \times 10^7$  c.g.s. units for the ratio  $e/m_0$ ; a value which agrees, in Kaufmann's words, 'quite well with that found for cathode rays [ $1.865 \times 10^7$ ]',<sup>4</sup> This is indeed within the limit of 5%, but, as we have noted, Simon's result, to which Kaufmann here referred, was not accurate.

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1. Boorse and Motz, 1966, p.511.

2. Ibid., p.512.

3. Ibid., p.509.

4. Ibid., p.510.

Although Kaufmann appeared to be satisfied with the method he had conceived, he certainly was not happy with its results. He was a strong proponent of the electromagnetic conception of nature and he presumably considered his experimental result a stumbling block. At the 1901 meeting of the German Natural Scientists and Physicians, he outlined his expectations for an electromagnetic physical science. He concluded that before such a view could be endorsed, one would have to consider the following problems:

to show that the electron mass is entirely electromagnetic, to reduce mechanics to electromagnetism, to prove that matter is composed solely of electrons, to relate chemical periodicities to the stable dynamic arrangements of assemblies of electrons, and to experimentally confirm Wien's electron theory of gravitation.

Kaufmann's 1901 result that the "apparent" mass of cathode rays is three times smaller than their "true", mechanical mass, obstructed the very first step which his plan for attaining a comprehensive electromagnetic view of nature required. There is no wonder therefore why he envisaged in 1901 a structure and a charge distribution other than that suggested by Searle which would show the "true", mechanical mass of the electron to be zero.

It did not take much time for Kaufmann's expectation to materialize. Being influenced by Wien who - after withdrawing in 1900 from Hertz's programme of pursuing a unifying mechanical physics - had converted to the idea of electromagnetic foundation for mechanics,<sup>2</sup> and being intrigued by Kaufmann's experimental results, M. Abraham (1875-1922) sought a theory of electrons which would provide the basis for Wien's idea and account for Kaufmann's results. In 1902 he published his first paper on this subject, 'Dynamics of Electrons',<sup>3</sup> in which he attempted to deduce a system of electrodynamics from the differential equations of the electromagnetic field.

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1. Quoted by McCormach, 1970a, p.481 (my emphases).

2. Wien, 1901.

3. Abraham, 1902,a; cf., Sci.Abs., A 6(1903), p.238 art.644.

As pointed out by Kaufmann, central to this task was the question: could the mass of an electron be entirely explained in terms of the dynamical actions of its electromagnetic field without resorting to a mass which exists independently of the electric charge? 'Only when this question is affirmatively answered,' Abraham remarked, 'can the possibility of a purely electromagnetic foundation for mechanics be recognized.'<sup>1</sup>

The starting point of Abraham's study was to examine critically the theoretical investigation of Searle upon which Kaufmann had relied. In pursuing this study he reached the important conclusion that the field energy determines only the 'longitudinal mass', that is, the resistance to acceleration in the line of motion. What resists the acceleration perpendicular to the line of motion is, according to Abraham, the 'transverse mass' which, he claimed, the electron's electromagnetic momentum determines. Since these two masses are different functions of the electromagnetic momentum, different accelerations would arise from the same force depending on whether it is exerted in a direction parallel or perpendicular to the line of motion. For slowly moving electrons, when the momentum is proportional to the velocity, the two masses are equal; but in general, according to Abraham, they are different. In other words, mass is not a scalar as classical mechanics has it, but rather a vector quantity of the nature of a tensor with the symmetry of an ellipsoid of revolution.<sup>2</sup>

On the basis of this analysis it became clear to Abraham that Kaufmann had not employed the correct formula. As the forces in Kaufmann's experiment were exerted perpendicularly to the electrons' direction of motion, it was the transverse mass that had to be considered and not the longitudinal one which Kaufmann, drawing upon Searle's theoretical investigation, had actually calculated.

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1. Quoted by Goldberg, 1970-1971,a, p.12.

2. Abraham, op.cit. Cf., Goldberg, ibid., pp.13-14; Jammer, 1961, p.151.

To arrive at the exact expression for the electromagnetic transverse mass, assumptions concerning the structure of the electron and its charge distribution had to be made. Abraham assumed that the electron is a rigid sphere whose charge is distributed uniformly either on its surface or in its volume. Calculating then the electromagnetic transverse mass, he obtained:

$$m_t(\text{Abraham}) = \frac{3}{4} m_0 \frac{1}{\beta^2} \left[ \left( \frac{1+\beta^2}{2\beta} \right) \ln \left( \frac{1+\beta}{1-\beta} \right) - 1 \right],$$

$$m_t(\text{Abraham}) = m_0 (1 + (2/5) \beta^2 + (9/35) \beta^4 + \dots),$$

where  $\beta$  is  $v/c$  and the power series represents an expansion for  $\beta \ll 1$ .<sup>1</sup> The velocity function is therefore,

$$\text{Abraham: } \bar{\phi}(\beta) = \frac{3}{4} \frac{1}{\beta^2} \left[ \left( \frac{1+\beta^2}{2\beta} \right) \ln \left( \frac{1+\beta}{1-\beta} \right) - 1 \right].$$

On the basis of this formula it is possible to calculate the ratio  $s$  between the electromagnetic transverse masses of two different electrons with the known velocities  $v_1$  and  $v_2$ , namely,  $s = m_t(v_1)/m_t(v_2)$ . The ratio  $r$  of the effective transverse masses for such electrons can be found from Kaufmann's experimental results; this ratio is  $r = (m+m_t(v_1))/(m+m_t(v_2))$ , where  $m$  is the mechanical mass. By eliminating  $m_t(v_2)$ , one obtains  $m/m_t(v_1) = (s-r)/s(r-1)$ . If the experimental ratio  $r$  were to coincide, within the limits of experimental error, with the theoretical ratio  $s$ , then the mechanical mass  $m$  could be considered zero.<sup>2</sup> Thus, if the experimental data of Kaufmann were to reveal for the effective transverse mass,  $m+m_t(v)$ , the same velocity dependence as for the electromagnetic transverse mass,  $m_t(v)$ , then the mechanical mass  $m$  would be necessarily zero.<sup>3</sup>

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1. For a detailed analysis of Abraham's theory of electrons see, Miller, 1981, pp.55-61, 109-12; Cushing, 1981, pp.1135-37.
  2. Jammer, 1961, pp.149-50; Lorentz, 1952, p.41.
  3. Jammer, *ibid.*, p.149.

In the light of this theoretical analysis, Abraham examined afresh Kaufmann's data and established that there was an agreement between his predictions and the experimental results. Notwithstanding the considerable uncertainty of Kaufmann's 1901 experiment, Abraham confidently concluded that 'the inertia of the electron is caused exclusively by its electromagnetic field'.<sup>1</sup>

Kaufmann immediately took up this new theoretical perspective. Working closely with Abraham at Göttingen, he re-did the experiment in an attempt to secure more accurate results; he presumably hoped that the new results would verify Abraham's theory and thereby render Searle's formula inadequate. / In his 1902 paper, 'Electromagnetic Mass of Electrons',<sup>2</sup> he abandoned Searle's formula and adopted Abraham's theory. On the basis of this new analysis, Kaufmann arrived at the conclusion that the mass of the electron is purely an electromagnetic phenomenon.

The collaboration of Abraham, the theoretician, and Kaufmann, the experimenter, came to the fore in September 1902 when they read sequential papers at the 74th meeting of the Natural Scientists at Karlsbad. Kaufmann conceded in the paper he presented that his previous experiments, particularly those of 1901, were not in a very good agreement with Abraham's theory.<sup>3</sup> He however reported that his recent experiment, accurate to about 1%–1.4%, did confirm Abraham's theory. Although, as he himself pointed out, a 2% error in the measurement of the velocity of the electron would amount to 19% error in the determination of its mass,<sup>4</sup> he confidently concluded that the dependence of the mass on its velocity is 'exactly represented by the Abraham formula'.<sup>5</sup> He then reiterated his view that 'the mass of the electron is

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1. Quoted by Goldberg, 1970–1971,a, p.14.

2. Kaufmann, 1902,a.

3. Kaufmann, 1902,b, p.54.

4. For  $\beta$  close to unity,  $\bar{\Phi}(\beta)$  varies very rapidly.

5. Quoted by Goldberg, 1970–1971,a, p.15.



of a purely electromagnetic nature'.<sup>1</sup> In the ensuing discussion Kaufmann referred theoretical questions to Abraham. Subsequent to that discussion Abraham presented the paper, 'Principles of Dynamics of Electrons',<sup>2</sup> in which he maintained with Kaufmann that the data strongly suggested that the mass of the electron was entirely electromagnetic. Naturally, in his turn Abraham referred questions on design and experimental technique to Kaufmann.<sup>3</sup>

The conspicuous atmosphere of success which Abraham and Kaufmann were creating at that time was quite understandable; in 1902 Abraham's theory was the only published theory that could quantitatively predict and account for the dependence of mass on velocity. It was only two years later, in 1904, that the success of Abraham and Kaufmann was challenged.

However, in 1903 Abraham was sufficiently assured of the correctness of his theory that he went as far as to reveal that his initial results had not agreed too well with Kaufmann's 1902 findings. He reported that Kaufmann had since then found a mistake in his calculation and improved the method of measuring the electric field strength.<sup>4</sup> According to Abraham the new results confirmed the view that 'the mass of the electron is purely of electromagnetic nature'.<sup>5</sup> On his part, Kaufmann extended the experimental conclusion - obtained only for Becquerel rays - to include cathode rays as well;<sup>6</sup> a result which H. Starke claimed later in that year to have confirmed experimentally.<sup>7</sup>

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1. Ibid. Cf., Cushing, 1981, p.1141.

2. Abraham, 1902,b; cf., Sci.Abs., A 6(1903), p.239 art. 645.

3. Goldberg described this collaboration as a symbiosis. (Goldberg, op.cit., p.7.)

4. Goldberg, ibid., p.17; Miller, 1981, pp.61-66; Cushing, 1981, p.1141.

5. Quoted by Goldberg, ibid.

6. Kaufmann, 1903; cf., Sci.Abs., A 6(1903), p.521 art.1411.

7. Starke, 1903; cf., Sci. Abs., A 7(1904), p.95 art.344. See also Miller, 1981, pp.64-67.

The so-called success of Kaufmann and Abraham came to a halt when in 1904 rival theories started to emerge. Exploring the possibility of a deformable electron which would conform to the Lorentz-FitzGerald contraction hypothesis, Lorentz arrived at a different, and mathematically simpler, formula from that of Abraham for the dependence of the transverse mass of the electron on its velocity:

$$m_t(\text{Lorentz}) = m_0(1 - \beta^2)^{-1/2},$$

$$m_t(\text{Lorentz}) = m_0(1 + (1/2)\beta^2 + (3/8)\beta^4 + \dots),$$

where  $\beta$  is  $v/c$  and the power series represents an expansion for  $\beta \ll 1$ .<sup>1</sup> The velocity function is therefore;

$$\text{Lorentz: } \bar{\Phi}(\beta) = (1 - \beta^2)^{-1/2}.$$

Abraham's objection to Lorentz's theory was categorical; in his view such a theory would undermine the possibility of obtaining an electromagnetic foundation for mechanics. He argued that, owing to the deformation of the electron in Lorentz's theory, some mechanical work would have to be performed to preserve the electron's stability; that in turn would require a non-electromagnetic inner force, quite apart from the internal electric force of the rigid electron theory. Consequently, a purely electrodynamic interpretation of Becquerel rays would be rendered untenable and with it the electromagnetic view of nature would have to be discarded. Abraham conceded that Lorentz's formulae for the electron mass are mathematically simpler than his own; however, he stressed that the requirement of Lorentz's theory for an additional, non-electromagnetic force would make that theory, from the physical point of view, far more

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1. H.A. Lorentz, 'Electromagnetic Phenomena in a System Moving with any Velocity less than that of Light', in Lorentz et al., 1952, p.24. Cf., Miller, *ibid.*, pp.73-74; Cushing, 1981, p.1138.

complicated than his own theory of rigid electrons.<sup>1</sup> It is an indication of the spirit of the time that Abraham considered this argument - namely, that Lorentz had failed to solidify the electromagnetic viewpoint - a valid form of criticism.<sup>2</sup>

In order to obviate the need for an additional force and yet to maintain a theory of deformable electrons, A.H. Bucherer (1863-1927) assumed in 1904 - as P. Langevin (1872-1946) independently did a year later<sup>3</sup> - that if the electron were to contract in the course of its motion in accordance with the Lorentz transformation, its volume would remain invariant under these deformations.<sup>4</sup> On the basis of this view that the electron is deformable but incompressible, Bucherer obtained another set of formulae, entirely different from that of Abraham and Lorentz, for the dependence of mass on velocity:

$$m_t(\text{Bucherer}) = m_0(1 - \beta^2)^{-1/3},$$

$$m_t(\text{Bucherer}) = m_0(1 + (1/3)\beta^2 + (2/9)\beta^4 + \dots),$$

where  $\beta$  is  $v/c$  and the power series represents an expansion for  $\beta \ll 1$ .<sup>5</sup> The velocity function is therefore,

$$\text{Bucherer: } \bar{\Phi}(\beta) = (1 - \beta^2)^{-1/3}.$$

To these three different theories there was yet another one to be added in 1905 and although it agrees formally with that of Lorentz, it

1. Abraham, 1904, pp.576-79. Cf., Goldberg, 1970-1971,a, pp.20-21; McCormmach, 1970,a, p.480; Jammer, 1961, p.147; Miller, *ibid.*, pp.75-79, 117-18.
2. Lorentz responded by noting Abraham's objection and conceding that it was certainly true that he, that is, Lorentz, had not shown that the electron, when deformed to an ellipsoid by its translation, would be in a stable equilibrium. However, in Lorentz's view the hypothesis of the deformable electron 'need not be discarded for this reason'. (Lorentz, 1952, p.214, emphasis in the original.)
3. Bucherer, 1904, pp.57-58; 1905. Langevin, 1905.
4. McCormmach, 1970,a, p.480.
5. Cushing, 1981, p.1138.

did revolutionize the whole subject. Einstein's celebrated 1905 paper, 'On the Electrodynamics of Moving Bodies',<sup>1</sup> laid down first principles which obviated the need to assume some electron structure and charge distribution. The two principles: the principle of relativity and the constant nature of the velocity of light in empty space independent of the state of motion of the emitting source, transformed electron theory; the arguments being kinematic rather than dynamic. Einstein's theory is a comprehensive theory whose generality by far exceeds that of the theories of Abraham and Bucherer.

As Pauli observed in his article on the theory of relativity, Einstein's theory 'constituted a definite progress that Lorentz's law of the variability of mass could be derived from the theory of relativity without making any specific assumptions on the electron shape or charge distribution. Also,' Pauli continued, 'nothing need be assumed about the nature of the mass.'<sup>2</sup> And he concluded that 'the old idea that one could distinguish between the constant "true" mass and the "apparent" electromagnetic mass, by means of deflection experiments on cathode rays, can... not be maintained.'<sup>3</sup>

However, it appears that as late as 1910 many physicists did not perceive the general character of Einstein's theory and did not take up the fundamental issues it had raised. At that time the topical question was not whether the two principles of Einstein's theory and their implications are valid or not, but rather could one construe mass as an entirely electromagnetic phenomenon and, moreover, attain a coherent electromagnetic conception of nature? Seen in that perspective, Einstein's theory was considered

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1. Einstein, 1905; references are to the English translation in Lorentz et al., 1952, pp.37-65. Miller pointed out that this translation is faulty and went on to produce a new one. However, as my argument is not affected by this new translation I shall be referring to the old one. (Miller, 1981, pp.391-415.)

2. Pauli, 1967, pp.82-83.

3. Ibid.

conservative: a mere generalization of Lorentz's theory.<sup>1</sup>

Nevertheless, the new theoretical developments stimulated Kaufmann to conduct a new set of experiments. While Abraham was engaged in defending the results on the theoretical front,<sup>2</sup> Kaufmann improved upon the experimental technique. Having realized the importance of high-vacuum techniques to an experiment such as his, Kaufmann investigated new methods and eventually succeeded in designing the first rotary high-vacuum pump.

Kaufmann published in 1905 a description of that new pump; essentially, it consisted of a glass cylinder around which two spiral tubes, connected to the cylinder, coiled. Rotation of the cylinder round its axis caused the mercury in the two spirals to rise and fall alternately, and thus to act as in the usual fall-pump.<sup>3</sup> Although, as Boorse and Motz pointed out, 'the pump was extremely fragile, unwieldy, and temperamental',<sup>4</sup> Kaufmann claimed to have used it successfully in his 1905-1906 experiments.<sup>5</sup>

The new set of experiments - experiments in which Kaufmann implemented improved measuring methods designed to obtain more accurate results - constituted the second phase of Kaufmann's researches into the nature of the electron. Whereas in the first phase Kaufmann had eventually tested one single theory in which he appeared to believe, namely, Abraham's theory, in the second phase his task was to discriminate between the various theories that had been suggested during the years 1904-1905. Yet, his experimental method was essentially the same as in his early experiments.

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1. Hirosige, 1968, pp.41-42. Cf., McCormach, 1970,b, pp.60-61. Miller pointed out that 'Einstein's relativity paper was considered as important mostly for the wrong reasons'. (Miller, 1981, p.235.)
  2. Abraham, 1904. Cf., Pyenson, 1979, pp.70-74.
  3. Kaufmann, 1905,a; cf., Sci.Abs., A 8(1905), p.537. See also Kaye, 1927, p.81.
  4. Boorse and Motz, 1966, p.505.
  5. Kaufmann, 1906,a, p.501.

Kaufmann reported on his new experimental work in 1905 and published the final account in 1906.<sup>1</sup> He opened his 1906 paper with a theoretical discussion of the various models, claiming that Einstein's formulae amounted to those of Lorentz; indeed, he regarded Lorentz's and Einstein's theories as two theories which assume the same electron model: the Lorentz-Einstein theory of electron.<sup>2</sup> Concluding the theoretical introduction, Kaufmann anticipated the general result of the measurements he was about to describe; he stated that

the results of the measurements are not compatible with the Lorentz-Einstein fundamental assumption. The Abraham and the Bucherer equations depict equally well the results of the observations. For the present a decision between these two theories by a measurement of the transverse mass of  $\beta$ -rays appears to be impossible.<sup>3</sup>

He then proceeded to describe the experiment and to discuss a method of analysing the data which in his view would force a decision between the various models.

According to Kaufmann the equations for the motion of the electron given by Lorentz and Abraham differ very considerably, and he expressed therefore some surprise that, as he wrote, 'an application of the equations to my earlier measurements by Herr Lorentz led to the... result, that my observations could be represented by him with the same accuracy as by the Abraham equations for the rigid electron'.<sup>4</sup> However, a comparison of the corresponding power series for  $m_t$  shows that the differences between these theories is small:

	$m_t$ (for $\beta=1$ )	$m_t$ (for $\beta=1/2$ )
Abraham	1.657 $m_0$	1.116 $m_0$
Lorentz	1.875 $m_0$	1.148 $m_0$
Bucherer	1.555 $m_0$	1.097 $m_0$

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1. Kaufmann, 1905,b, and Kaufmann, 1906,a.

2. Kaufmann, 1905,b, p.954; Kaufmann, 1906,a, pp.487-95, 530-31. Kaufmann considered Einstein's theory to be based on just one principle, namely, the principle of relative motion. Cf., Miller, 1981, pp.226ff., 329.

3. Kaufmann, 1906,a, p.495.

4. Ibid., p.493 (my emphases). See also *infra*, pp.337-38.

Nevertheless, given sufficient accuracy, there was a possibility of distinguishing experimentally between the various theories. Indeed, Kaufmann pointed out that there is a 5%-7% difference between the velocities Abraham's and Lorentz's theories yield for each measured curve-point. Hence, 'a way was provided for differentiating between the two theories'.<sup>1</sup> Naturally, as Kaufmann remarked, from the outset there was the possibility of neither theory providing sufficiently accurate agreement.<sup>2</sup>

Kaufmann's method consists in determining several constants of the actual curve, and comparing them with the corresponding constants obtained from the various theories. Clearly, the measured constants - in Kaufmann's terminology, the Apparatkonstanten - unlike the calculated constants, that is, the Kurvenkonstanten, were independent of any specific assumptions concerning the structure of the electron and its charge distribution; rather, they were functions of the strengths of the electric (E) and magnetic (M) fields and the value of  $e/m_0$ , that is, the ratio  $e/m$  for slowly moving electrons, like those that constitute cathode rays.<sup>3</sup> Such a comparison could serve therefore as a criterion for the correctness of the theories; however, it was a very delicate process. When Kaufmann presented one set of readings (Table VIII), he remarked that the Table indicates that 'from the shape of the curves alone, without taking into consideration the absolute values of the constants, it is not possible to come to any decision in favour of one or the other theory'.<sup>4</sup>

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1. Kaufmann, *ibid.*

2. *Ibid.*

3.  $1/A = (e/m_0)(M/c);$   $B = (e/m_0)(E/c^2);$   
 $C = AB = E/Mc;$   $D = (E/M^2)(m_0/e);$   
 $e/m_0 = (cC/D)(cC/ME)^{1/2};$   
 $C = \beta (y'/z'); \quad y'^2 = C^2 z'^2 + D^2 z'^4$

where  $y'$  and  $z'$  are the reduced coordinates  $y_0$  and  $z_0$  (see *infra* p.307 footnote no. 2), and  $c$  the velocity of light in vacuum. See Kaufmann, 1906, a, pp.493, 530, 531, 534. Cf., Miller, 1981, pp.228-30.

4. Kaufmann, *ibid.*, p.532.

Table VIII<sup>1</sup>

Comparison between the Observed and the Calculated Curve

$z'$	$y'_{\text{beob.}}$	$p$	$y'_{\text{ber.}}$			$\delta$			$\beta$		
			I	II	III	I	II	III	I	II	III
0,1350	0,0246	0,5	0,0251	0,0246	0,0254	- 5	0	- 8	0,974	0,924	0,971
0,1919	0,0376	1	0,0377	0,0375	0,0379	- 1	+ 1	- 3	0,922	0,875	0,919
0,2400	0,0502	1	0,0502	0,0502	0,0502	0	0	0	0,867	0,875	0,864
0,2890	0,0545	1	0,0649	0,0651	0,0647	- 4	- 6	- 2	0,807	0,823	0,805
0,3359	0,0811	1	0,0811	0,0813	0,0808	0	- 2	+ 8	0,752	0,713	0,750
0,3882	0,1001	1	0,0995	0,0997	0,0992	+ 6	+ 4	+ 9	0,697	0,661	0,695
0,4305	0,1205	1	0,1201	0,1202	0,1200	+ 4	+ 3	+ 5	0,649	0,616	0,647
0,4735	0,1405	0,25	0,1408	0,1405	0,1409	- 3	0	- 4	0,610	0,579	0,605
0,5252	0,1667	0,25	0,1682	0,1678	0,1687	-15	-11	-20	0,568	0,527	0,564

$z'$  and  $y'_{\text{beob.}}$  are the reduced coordinates,  $z_0$  and  $y_0$ .<sup>2</sup>  $p$  is the assigned weight: a measure of the reliability of each reading. The three columns: I, II and III, under  $y'_{\text{ber.}}$  represent the calculated results according to the theories of Abraham, Lorentz and Bucherer respectively.  $\delta \times 10^{-4}$  represents the difference between  $y'_{\text{beob.}}$  and  $y'_{\text{ber.}}$ , that is, the difference between the observed and the calculated value for  $y'$ .  $\beta$  is the ratio  $v/c$  associated with the coordinates  $(y_0, z_0)$  according to the respective theory.

Prima facie it appeared, as Kaufmann put it, that 'one can merely say for the present that all three theories represent equally well the relative shape of the curve'.<sup>3</sup> However, he pointed out that

from the values for  $\beta$  it is gathered ... that Lorentz's theory demands quite different velocities than the theories of Bucherer and Abraham. For both of these the velocities are almost identical, which

1. Ibid.

2. To facilitate the theoretical analysis of the resulting curve, Kaufmann introduced certain approximations which required the use of deflections substantially smaller than the apparatus' dimensions. As the actual observational results, namely,  $(y_0, z_0)$ , did not comply with this stipulation, Kaufmann had to reduce them to 'infinitely small deviations'. (For the method of reduction see *ibid.*, pp.524-30. Cf., Miller, 1981, pp.62-65, 228-29; Cushing, 1981, p.1142.)

3. Kaufmann, *ibid.*, pp.532-33.



is still more worthy of note: the function  $\tilde{\Phi}(\beta)$  agrees numerically, as one can easily convince oneself, for both within the velocity-interval here under consideration, with a deviation of at most 2 per cent.<sup>1</sup>

On the basis of one comparative method - a method which did not depend on the value of  $e/m_0$ <sup>2</sup> - Kaufmann calculated that Abraham's and Bucherer's theories showed discrepancies of 3.5% and 2.8% respectively, in contrast to Lorentz's model which exhibited a discrepancy of 10.4%.<sup>3</sup> Apparently, Kaufmann was not satisfied with a conclusion based on just one type of comparison, for he obtained a second comparison which was dependent on the value of  $e/m_0$ . This value could be obtained, on the one hand, from the direct relation between one particular theory and the observed curve of the deflected  $\beta$ -rays, that is, from the 'curve constants', and, on the other hand, from Simon's measurement of deflected cathode rays. Thus the calculated values of  $e/m_0$ , derived from the different theories, could be compared on independent ground with the experimental value of  $e/m_0$ . Kaufmann, however, did not use Simon's 1899 experimental results, i.e.,  $1.865 \times 10^7 \text{ emug}^{-1}$ ; employing the three theoretical formulae, he extrapolated Simon's value to infinitely slow electrons and obtained a mean value of  $1.878 \times 10^7 \text{ emug}^{-1}$ .<sup>4</sup> It is worth mentioning again that the accepted value of  $e/m_0$  is  $(1.75890 \pm 0.00002) \times 10^7 \text{ emug}^{-1}$ , 6.3% lower than Kaufmann's extrapolated value of Simon.

Kaufmann calculated that according to Abraham's theory the mean value of  $e/m_0$  was  $1.823 \times 10^7$ , with a spread of 1.9%; on the basis of Bucherer's

1. Ibid., p.533.
2. By considering the product of two 'apparatus constants', Kaufmann could refer to a constant which is independent of  $e/m_0$ :  $C = AB = E/Mc$ . (Ibid., p.531. Cf., supra, p.306 footnote no.3; Miller, 1981, p.230.)
3. Kaufmann, ibid., p.533. Cf., Miller, ibid. (There is a printer's error in the presentation of the discrepancies: -2.5% should be -2.8%.)
4. Kaufmann, ibid., pp.533, 548-51. Cf., Miller, ibid.; Cushing, 1981, p.1142 (for  $v=0$ ,  $e/m_0 = 1.878 \times 10^7 \text{ emug}^{-1}$  and not  $1.885 \times 10^7$  as it appears in Cushing's paper).

theory the mean value was  $1.808 \times 10^7$ , with a spread of 1.4%; and, finally, Lorentz's theory resulted in a mean value of  $1.660 \times 10^7$ , with a larger spread of values around the mean: 5.5%.

Comparing these values with the extrapolated value of Simon, Kaufmann found that Abraham's and Bucherer's theories gave rise to discrepancies of 2.9% and 3.7% respectively, by far smaller than a discrepancy of 11.6% which Lorentz's theory exhibited.<sup>1</sup> However, if one compares these values with the accepted value for  $e/m_0$ , one naturally arrives at different discrepancies: Abraham's and Bucherer's theories show discrepancies of 3.6% and 2.8% respectively, whereas Lorentz's theory reduces its discrepancy by more than a half to 5.6%.

#### First Comparison

Abraham	-3.5%
Bucherer	-2.8%
Lorentz	-10.4%

#### Second Comparison ( $e/m_0 \times 10^{-7}$ )

	Max v.	Min v.	Mean v.	Spread	Simon's ext. v.: 1.878	accepted v.: 1.759
Abraham	1.858	1.788	1.823	1.9%	-2.9%	+3.6%
Bucherer	1.833	1.780	1.808	1.4%	-3.7%	+2.8%
Lorentz	1.751	1.569	1.660	5.5%	-11.6%	-5.6%

The possibility that Simon's extrapolated value for  $e/m_0$  might introduce a constant error into the second comparison did not escape Kaufmann's eyes. Indeed, he stressed that no complete agreement had appeared to prevail concerning the value of  $e/m_0$ .<sup>2</sup>

Kaufmann's third and final test of the contesting theories consisted in calculating from the 'apparatus constants' - using again the extrapolated

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1. Kaufmann, *ibid.*, p.533.

2. *Ibid.*, p.531.

value of Simon's result and convenient values of  $z'$  - the theoretical values of  $y'$  for each theory. A comparison of the calculated curves with the reduced observed curve showed that Lorentz's calculated curve deviated most: the greatest difference between Abraham's calculated results and those of Lorentz being of about 5%.<sup>1</sup>

### Third Comparison

$z'$	$y'$		
	Abraham	Bucherer	Lorentz
0.1	0.0191	0.0190	0.0196
0.2	0.0413	0.0407	0.0434
0.3	0.0712	0.0696	0.0745
0.4	0.1104	0.1080	0.1144
0.5	0.1595	0.1568	0.1642

As a careful experimenter Kaufmann once again called the reader's attention to the fact 'that the last comparison... is based on the assumption that the Simon value of  $e/m_0$  is correct'.<sup>2</sup> However, the first comparison, according to Kaufmann, remained free from such an assumption.<sup>3</sup>

In Kaufmann's view the conclusion from these three comparisons was clear. He confidently maintained that 'the above results speak decisively against the correctness of Lorentz's theory and consequently also of that of Einstein's theory'.<sup>4</sup> He thus argued that 'if one were to consider these theories as thereby refuted, then the attempt to base the whole of physics, including electrodynamics and optics, upon the principle of relative motion would have to be regarded at present also unsuccessful'.<sup>5</sup> It is noteworthy that Kaufmann did not consider his experiment a direct test of Einstein's theory, though he was the first physicist to cite this celebrated 1905

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1, Ibid., p.534. Cf., Miller, 1981, p.231.

2. Kaufmann, *ibid.*

3. Ibid.

4. Ibid.

5. Ibid.

relativity paper; rather, he intended his experiment to be a test of three models of the electron.<sup>1</sup> The purported refutation of Einstein's theory is considered, in this final conclusion of Kaufmann, a consequence of the incompatibility of Lorentz's electron model with the experimental results.<sup>2</sup>

Kaufmann's concession that his experiment was not sufficiently accurate, to discriminate between the theories of Abraham and Bucherer did not weaken, let alone undermine, his view that the experiment demonstrated the failure of Lorentz's theory, and consequently that of Einstein. He thus upheld the assumption that 'physical phenomena depend on the movement relative to a quite definite coordinate system which we call the absolut ruhenden Äther'.<sup>3</sup> The fact that it had not been possible to demonstrate by electrodynamic or optical experiments the existence of this fixed coordinate system - that is, the absolute resting ether - did not deter Kaufmann from concluding that 'a decision may not be made as to the impossibility of such a proof'.<sup>4</sup>

Kaufmann neither retracted his conclusions nor carried out new experiments to test Abraham's and Bucherer's theories.<sup>5</sup> The most he was prepared to concede - in a discussion with Planck - was that both Lorentz's theory and that of Abraham did not agree with the experimental results; indeed, he was willing to entertain the possibility of small observational errors occurring in such a way as to increase the deviations between theory and experiment. Notwithstanding, he stressed that whereas Abraham's theory

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1. Miller, 1981, pp.226-27, 343.

2. Ibid. Cf., Hirose, 1968, pp.41-42; Hirose, 1976, p.75; Holton, 1975, pp.234-35.

3. Kaufmann, 1906,a, p.535.

4. Ibid.

5. According to Kaufmann the main obstacles for increasing the accuracy would stem from the photographic plate, namely, the behaviour of the emulsion under the required experimental conditions. Other problems would arise from maintaining a constant electric field in a scaled up apparatus and measuring accurately the magnetic field. In Kaufmann's view 'a further increase in the accuracy... would call for means which far outstrip the current possibilities of the Institute'. (Ibid.)

deviated 3%-5% from the observed data, that of Lorentz deviated 10%-12%.<sup>1</sup> A year later, in 1907, Bestelmeyer obtained a lower experimental result for  $e/m_0$ : 7.8% less than that of Simon and 8.5% less than the extrapolated value Kaufmann had calculated.<sup>2</sup> In view of this finding, Planck resumed his criticism and arrived at the conclusion that the electric field might have varied as a result of an ionizing effect.<sup>3</sup> Nevertheless, in his reply Kaufmann upheld his conclusion and strongly argued that an 8% variation in the electric field was not possible in his set-up; at all events, any small variation would not alter the result that the data proved the Lorentz-Einstein theory incorrect.<sup>4</sup>

Kaufmann's results aroused much interest and received strictures as well as support from many distinguished physicists. These varied reactions are of considerable importance since they show how differently the same experimental results can be apprehended. Moreover, they throw light on the relation between what is perceived as an experimental error and the epistemological context within which such a perception is made. The dependence of error on epistemic system is discernible in such a relation and it is to this issue that I now turn by discussing three different responses which Kaufmann's experimental results received.

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1. Kaufmann, 1906,b. Cf., Miller, 1981, p.233.

2. Bestelmeyer, 1907. Cf., Miller, *ibid.*, pp.335-40.

3. Planck, 1907.

4. Kaufmann, 1907. Cf., Miller, 1981, pp.340-41.

## 6.2 Poincaré's Reaction

In the introduction to his book, Science and Method, Poincaré observed that 'mechanics seem to be on the point of undergoing a complete revolution. The ideas which seemed most firmly established are being shattered by daring innovators'.<sup>1</sup> It is natural to think that by 1908, when the book was published, Einstein would have been already considered one of those innovators. However, to Poincaré, as S. Goldberg suggests, Einstein's work appeared as a small and rather insignificant part of a much broader theory, namely Lorentz's theory, which had been developed from 1892 under the guiding criticism of Poincaré to its completion as a theory of electrons in 1904.<sup>2</sup> Indeed, Einstein is not mentioned at all in this celebrated book of Poincaré.<sup>3</sup> Poincaré's heroes, so to speak, were those who sought a new theory of matter; a theory which would explain the phenomenon of matter solely in terms of electrons immersed in ether: 'beyond the electrons and the ether', Poincaré proclaimed, 'there is nothing'.<sup>4</sup>

It seems that as Einstein set himself a completely different problem he was not one of Poincaré's heroes. Einstein questioned the nature of space and time, and the meaning of simultaneity; he simply was not concerned with ontology. As Zahar remarked, Einstein had implicitly posited 'a domain of events, each of which can be referred to by coordinates (t, x, y, z,) in any one of infinitely many equivalent inertial frames. Events are therefore the constituents of the Einsteinian universe'.<sup>5</sup> Hence Einstein's

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1. Poincaré, 1908, p.11.

2. Goldberg, 1970-1971,b, p.84; Goldberg, 1970, p.97.

3. Poincaré wrote of Einstein - shortly after they met in 1911 at the Solvay conference - that he 'is one of the most original thinkers I have ever met... Since he seeks in all directions, one must... expect the majority of the paths on which he embarks to be blind alleys'. (Quoted by Miller, 1981, p.255.) On the relationship between Poincaré and Einstein see Pais, 1983, pp.169-72.

4. Poincaré, 1908, p.209.

5. Zahar, 1973, pp.122-23.

interest in the way things are measured; altogether a different interest from that of Poincaré who was intent on actually measuring things.<sup>1</sup> It comes therefore as no surprise that Poincaré praised the theoretical work of Lorentz and Abraham and the experiments of Kaufmann, even when their results were not in agreement with his own ideas; after all, these researches focused on the very problem Poincaré himself was interested in.

Thus, as early as 1902, Poincaré described Lorentz's theory as 'the most satisfactory theory...; it is unquestionably the theory that best explains the known facts, the one that throws into relief the greatest number of known relations, the one in which we find most traces of definitive construction'.<sup>2</sup> According to Poincaré, 'the only object of Lorentz was to include in a single whole all the optics and electro-dynamics of moving bodies'.<sup>3</sup> This objective was much in keeping with Poincaré's maxim: 'the true and only aim [of science] is unity'.<sup>4</sup>

Referring to the theoretical work of Abraham and the experimental results of Kaufmann, Poincaré remarked in 1905 that they 'have... shown that the mechanical mass... is null, and that the mass of the electrons... is of exclusively electrodynamic origin'. This result, Poincaré continued, 'forces us to change the definition of mass; we cannot any longer distinguish mechanical mass and electrodynamic mass, since then the first would vanish; there is no mass other than electrodynamic inertia'.<sup>5</sup> It should be stressed that Poincaré did not show here any scepticism with regard to the experimental results; he considered Kaufmann's results, at least in 1905, conclusive.

Three years later this air of certainty still prevailed in the writings of Poincaré. Although the experimental results of Kaufmann were questioned

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1. Goldberg, 1967, p.944.

2. Poincaré, 1952, p.175.

3. Ibid., p.176. In Poincaré's view, Lorentz 'did not claim to give a mechanical explanation'. (Ibid.)

4. Ibid., p.177.

5. Poincaré, 1946, p.311.

at that time extensively, especially by Planck and Einstein,<sup>1</sup> and the relativity theory of Einstein had already started to establish itself as a comprehensive theory, Poincaré had still much to say in praise of the results of Lorentz, Abraham and Kaufmann. Poincaré remarked,

Abraham's calculations make us acquainted with the law in accordance with which the fictitious mass varies as a function of the velocity, and Kaufmann's experiment makes us acquainted with the law of variation of the total mass. A comparison of these two laws will therefore enable us to determine the proportion of the actual mass to the total mass.

In Poincaré's view, the result of Kaufmann's determination of this proportion is 'most surprising: the actual mass is nil'. And he concluded that

we have thus been led to quite unexpected conceptions. What had been proved only in the case of the cathode corpuscles has been extended to all bodies. What we call mass would seem to be nothing but an appearance, and all inertia to be of electromagnetic origin.<sup>3</sup>

Since Poincaré regarded the principle of relativity as a general law of Nature, a generalization from experience,<sup>4</sup> he felt compelled to extend the contraction hypothesis of Lorentz and FitzGerald - an hypothesis which becomes meaningful under this principle<sup>5</sup> - to the electrons themselves.

Notwithstanding Abraham's consideration that electrons are spherical and undeformable, Poincaré was of the opinion that 'we shall have to admit that the electrons, while spherical when in repose, undergo Lorentz's contraction when they are in motion, and then take the form of flattened ellipsoids'.<sup>6</sup> Two theories thus presented themselves: 'one in which the

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1. Planck, 1906; Planck, 1907. For a detailed discussion of Planck's response to Kaufmann's experimental results see Miller, 1981, pp.232-35, 340-41, 345; Cushing, 1981, pp.1142-46; Zahar, 1978. For Einstein's view, see *infra*, pp. 327ff.

2. Poincaré, 1908, p.206 (emphases in the original).

3. *Ibid.*, pp.206-7 (emphases in the original).

4. *Ibid.*, p.226; Poincaré, 1952, pp.135-39.

5. In his 1904 paper, Lorentz inferred the contraction hypothesis from considerations of a translation between two systems; in an earlier paper he had simply posited this hypothesis. (Lorentz et al., 1952, pp.4-7, 22-23.)

6. Poincaré, 1908, p.226.



electrons are undeformable, which is Abraham's; the other, in which they undergo Lorentz's deformation'.<sup>1</sup>

Poincaré sided with Lorentz and, with what has come to be known as 'Poincaré's stress', gave a theoretical underpinning to the mechanism of the Lorentz contraction and the stability of the electron under such deformation.<sup>2</sup> But the question still persisted: which is the correct theory?

As an exponent of conventionalism who maintained that 'the aim of science is not things themselves... but the relations between things',<sup>3</sup> and for whom 'science is only a classification and that... cannot be true, but convenient',<sup>4</sup> it is difficult to understand why Poincaré set himself the very trap which a realist would have gladly laid before a conventionalist, namely the pitfall of experimentum crucis. Whereas in 1902 Poincaré had considered experiment a guide which helps us in our free choice among all possible conventions,<sup>5</sup> in 1908 he considered its role to be that of an arbiter. 'The method employed by Kaufmann', he remarked, 'would... seem to give us the means of deciding experimentally between the two theories'.<sup>6</sup> And in the concluding remarks to his account of the new mechanics, he opined that 'further experiments will no doubt teach us what we must finally think of them (i.e., the new theories). The root of the question is in Kaufmann's

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1. Ibid., p.228.

2. 'One obtains... a possible explanation of the contraction of the electron by assuming that the deformable and compressible electron is subject to a sort of constant external pressure the action of which is proportional to the volume variation'. (Poincaré, 1905, p.1504. Quoted by Pais, 1983, p.158.) Acknowledging Poincaré's contribution, Lorentz remarked that it had made the mechanism of the contraction much clearer. (Lorentz, 1952, pp.213-14.) For a detailed discussion see Miller, 1973. Cf., Miller, 1981, pp.83-85.

3. Poincaré 1952, p.xxiv.

4. Poincaré, 1946, p.352.

5. Poincaré, 1952, p.50.

6. Poincaré, 1908, p.228 (emphasis in the original).

experiment and such as may be attempted in verification of it'.<sup>1</sup> It was perhaps unfortunate for Poincaré that precisely at the time Science and Method was going to the press, he learnt that Bucherer had made such an experiment and had in fact obtained results which confirmed, contrary to Kaufmann's results, Lorentz's theory.<sup>2</sup>

The question why did Poincaré, a noted conventionalist, confer so much weight on a single experiment to the point of believing that it, in his words, 'revolutionizes at once Mechanics, Optics, and Astronomy',<sup>3</sup> is at the centre of the difficulty which underlies Poincaré's reaction to Kaufmann's experiment. One way of solving this problem is to conceive, as Goldberg does, a distinction between Poincaré the philosopher and Poincaré the physicist, and to simply stipulate an antagonism between the two. According to Goldberg, Poincaré might well have assumed a conventionalistic position while talking about the nature of physics; however, in his work in theoretical physics 'he was anything but a conventionalist'.<sup>4</sup> Specifically, Goldberg argues that in the case of Kaufmann's experiment, Poincaré's reaction was an example of his wavering between conventionalism and realism.<sup>5</sup> This argument of Goldberg impoverishes Poincaré's philosophy and deprives the scientific achievement of Poincaré of a coherent epistemology.

To be sure, Poincaré was quite aware of such criticism; in fact, he anticipated it.

'Have you not written,' he invited his reader in 1905 to ask the author, i.e., Poincaré himself, 'that the principles [e.g. the principle of relativity and the principle of conservation of mass], though of experimental origin, are now unassailable by experiment because they have become conventions? And now you have just told us that the most recent conquest of experiment put these principles in danger'.<sup>6</sup>

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1. Ibid., p.249.

2. Ibid., p.228 (footnote). On Bucherer's experimental work see Miller, 1981, pp.345-49.

3. Ibid., p.286. See also Poincaré, 1946, p.545.

4. Goldberg, 1967, p.938.

5. Goldberg, 1970-1971, b, pp.76-77.

6. Poincaré, 1946, p.318.

Indeed, for Poincaré 'principles are conventions and definitions in disguise. They are, however, deduced from experimental laws, and these laws have, so to speak, been erected into principles to which our mind attributes an absolute value'.<sup>1</sup> In other words, the principle of relativity and the principle of conservation of mass, for example,

are results of experiments boldly generalized; but they seem to derive from their very generality a high degree of certainty. In fact, the more general they are, the more frequent are the opportunities to check them, and the verifications multiplying, taking the most varied, the most unexpected forms, end by no longer leaving place for doubt.<sup>2</sup>

But then, new experimental results did compel one to doubt the absolute value which had been attributed to these principles. Specifically, Poincaré himself questioned in 1905 that 'if there is no longer any mass, what becomes of Newton's law?... If the coefficient of inertia is not constant, can the attracting mass be? That is the question'.<sup>3</sup> And in 1908 he opined that Kaufmann's experiments 'have shown Abraham's theory to be right.' Accordingly, it would seem that the Principle of Relativity has not the exact value we have been tempted to give it'.<sup>4</sup> Poincaré's answer to this criticism is somewhat bemusing: 'Well, formerly I was right and today I am not wrong. Formerly I was right,' he emphasized, 'and what is now happening is a new proof of it.'<sup>5</sup>

A closer reading of Poincaré's writings may clarify this position and resolve the criticism it received. Poincaré's philosophy of science, as it is stated and explained in Science and Hypothesis, is indeed not a rigid conventionalism. Poincaré appears to discern in science a spectrum of philosophies. At one extreme lies conventionalism which forms the philosophical foundations of geometry, and at the other extreme lies induction:

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1. Poincaré, 1952, p.138.

2. Poincaré, 1946, p.301.

3. Ibid., p.312.

4. Poincaré, 1908, p.228 (emphases in the original).

5. Poincaré, 1946, p.318.

the method of the physical sciences. Poincaré located the science of mechanics in between these two extremes; this is a science in which the two methods, deduction and induction, operate in concert.

Thus, for Poincaré, 'geometrical axioms are... neither synthetic a priori intuitions nor experimental facts. They are conventions. Our choice among all possible conventions is guided by experimental facts; but it remains free ... In other words, the axioms of geometry... are only definitions in disguise'.<sup>1</sup> Though, according to Poincaré, experiment plays a considerable role in the genesis of geometry,

'it would be a mistake to conclude... that geometry is, even in part, an experimental science. If it were experimental, it would only be approximative and provisory ... Geometry would be only the study of the movements of solid bodies; but, in reality, it is not concerned with natural solids: its object is certain ideal solids, absolutely invariable, which are but a greatly simplified and very remote image of them... Experiment,' Poincaré concluded, 'tells us<sup>2</sup> not what is the truest, but what is the most convenient geometry.'

In contrast, at the other end of the spectrum, where according to Poincaré the physical sciences lie, induction is the guiding method. It is here that 'experiment is the source of truth'.<sup>3</sup> Here, as Poincaré put it, experiment 'alone can teach us something new; it alone can give us certainty'.<sup>4</sup> However, Poincaré qualified this strong view by remarking that all that experiment affirms 'is that under analogous circumstances an analogous fact will be produced'.<sup>5</sup> And even of that claim one is never absolutely sure; however founded a prediction may appear, it may prove, in Poincaré's view, baseless if one sets to varify it.<sup>6</sup> Furthermore, it was

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1. Poincaré, 1952, p.50 (emphases in the original).

2. Ibid., pp.70-71.

3. Ibid., pp.xxvi, 140.

4. Ibid.

5. Ibid., p.142.

6. Ibid., p.144.

clear to Poincaré that the requirement that experiments should be carried out without preconceived ideas is impossible. He observed that 'every man has his own conception of the world, and this he cannot so easily lay aside'.<sup>1</sup> An experimental law is therefore always subject to revision: 'one single piece of work by a real master', - Poincaré had Pasteur in mind - 'will be sufficient to sweep them [i.e. bad experiments] into oblivion'.<sup>2</sup> Thus, conventions and crucial experiments constitute the extremes of the spectrum and in between lies the science of mechanics, a science which combines both conventionalism, a deductive method, and experimentation, an inductive method.

'The principle of mechanics,' Poincaré argued, are 'presented to us under two different aspects. On the one hand, there are truths founded on experiment, and verified approximately as far as almost isolated systems are concerned; on the other hand,' Poincaré continued, 'there are postulates applicable to the whole of the universe and regarded as rigorously true. If these postulates possess a generality and a certainty which falsify the experimental truths from which they were deduced, it is because they reduce in final analysis to a simple convention that we have a right to make, because we are certain beforehand that no experiment can contradict it.'<sup>3</sup>

However, the conventions in the science of mechanics are not arbitrary.

Experiments have shown that, in Poincaré's words, 'it will be convenient' to generalize their results and to consider them as definitions. Experiments thus serve as a basis for mechanics, and yet, according to Poincaré, will never invalidate it.<sup>4</sup>

It is important to note that Poincaré did not seek - in this spectrum of philosophies and their respective sciences - to isolate and separate the various sciences. On the contrary, he warned that to raise a barrier between, for example, experimental mechanics and the conventional mechanics of general principles will mutilate both; furthermore, the remains of an

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1. Ibid., p.143.

2. Ibid., p.142; cf., ibid., p.95.

3. Ibid., pp.135-36.

4. Ibid., pp.105, 136.

isolated conventional mechanics will be very little and, as Poincaré argued, 'can in no way be compared with that grand body of doctrine which is called geometry'.<sup>1</sup>

In the light of this philosophical outlook, Poincaré's reaction to Kaufmann's experiment may become clearer. On the one hand, he considered it an experimentum crucis, an arbiter of theories, and as such he accepted, so to speak, its judgements; on the other hand, he was aware of its possible limitations, i.e. its approximations and possible errors. Hence his plea that before adopting Kaufmann's result 'some reflection is necessary. The question is one of such importance', he remarked, 'that one would wish to see Kaufmann's experiment repeated by another experimenter'.<sup>2</sup> However, in Poincaré's view, Kaufmann was a skilful experimenter who had taken 'all suitable precautions', and hence, 'one cannot well see what objection can be brought'.<sup>3</sup>

Nevertheless, there was one possible source of error which Poincaré was not prepared to ignore: the measurement of the electric field; 'the measurement upon which', in Poincaré's words, 'everything depends'.<sup>4</sup> In any deflection experiment in which uniform electric field is applied a high vacuum is required; Kaufmann's experiment was not an exception: a high vacuum had to be created between the two plates of the condenser, so that a truly uniform electric field would be obtained. 'Is this certain?' asked Poincaré. 'May it not be,' he questioned, 'that there is a sudden drop in the potential in the neighbourhood of one of the armatures, of the negative armature, for instance? There may be a difference in potential at the point of contact between the metal and the vacuum, and it may be that this difference is not the same on the positive as on the negative

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1. Ibid., p.138.

2. Poincaré, 1908, p.228.

3. Ibid., p.229. Poincaré's opinion of Blondlot is another example of this submission to authority. (See *supra*, p. 249 footnote no. 3.)

4. Ibid.

side... It would seem,' Poincaré concluded, 'that we must at least take into account the possibility of this occurring, however slight the probability may be'.<sup>1</sup>

In retrospect it appears that that was indeed the principal source of error which undermined Kaufmann's experiment.<sup>2</sup> However, as Poincaré was reluctant to suspend judgement on, let alone dismiss, an experiment of which he was rightly suspicious but whose importance he had recognized, he was not willing to pursue persistently possible sources of error and attempt to establish experimental errors that might have led Kaufmann astray. A theoretician, for whom experiments form the building blocks of knowledge – knowledge whose cement, so to speak, the inductive method provides and to whose overall architecture conventionalism attends – Poincaré relied heavily on Kaufmann's expertise and was prepared to concede, as late as 1908, that Abraham's theory had been shown to be right.

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1. Ibid.

2. See *infra*, p. 347.

### 6.3 Einstein's Reaction

In contradistinction to Poincaré who construed the principles of mechanics in general and the principle of relativity in particular as basically bold generalizations of a posteriori experience gleaned from several experiments, Einstein elevated the principles of his relativity theory to a priori postulates which stipulate the features of the laws of nature so that there are no special privileged observers. To be more specific, though Einstein spoke in his celebrated 1905 paper, 'On the Electrodynamics of Moving Bodies',<sup>1</sup> of 'unsuccessful attempts to discover any motion of the earth relatively to the "light medium"',<sup>2</sup> he did not specify them. In Einstein's view these experiments, about which he was notoriously vague,<sup>3</sup> 'suggest that the phenomena of electrodynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest'.<sup>4</sup> Hence, 'they suggest', as Einstein viewed it, that 'the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good'.<sup>5</sup> Thus far Einstein was prepared to go in locating the sources of his theory in experiments. His next move, of which he was quite explicit, was to raise this suggestion or, in Einstein's words, 'this conjecture... to the status of a postulate, and also [to] introduce another postulate,... namely, that light is always propagated in empty space with a definite velocity  $c$  which is independent of the state of motion of the emitting body'.<sup>6</sup> Thus, in Einstein's theory of relativity a suggestion of empirical nature dissociated itself completely from its experimental sources and attained the status of a postulate which, together with another

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1. Einstein, 1905, p.37.

2. Ibid.

3. See, for example, Holton, 'Einstein, Michelson, and the "Crucial" Experiment', 1975; Zahar, 1973, pp.231-32; Pais, 1983, pp.172-73; Miller, 1981, pp.143ff.

4. Einstein, 1905, p.37.

5. Ibid., pp.37-38.

6. Ibid., p.38.



postulate, provided the axiomatic base for 'a simple and consistent theory of the electrodynamics of moving bodies'.<sup>1</sup>

For Poincaré the principles of mechanics always remained attached to their origins in experiments. For him the principle of relativity is not only a necessary consequence of the hypothesis of central forces and is indeed confirmed by daily experience, but it is irresistibly imposed upon our good sense; 'and yet', as Poincaré put it, 'it is also assailed... [and] may well not have the rigorous value which has been attributed to it'.<sup>2</sup> Speculating on this issue Poincaré raised the question as to

'what would happen if one could communicate by non-luminous signals whose velocity of propagation differed from that of light? If, after having adjusted the watches by the optical procedure, we wished to verify the adjustment by the aid of these new signals, we should observe discrepancies which would render evident the common translation of the two stations. And are such signals inconceivable,' Poincaré speculated further, 'if we admit with Laplace that universal gravitation is transmitted a million times more rapidly than light?'<sup>3</sup>

Poincaré was thus prepared on both philosophical and experimental ground for a refutation of this principle.<sup>4</sup> By comparison, for Einstein a refutation did not only mean the rejection of a single principle, but also the discarding of a comprehensive theory which, on the one hand, is simple and clear in terms of its logical deductions and, on the other, embraces a wide complex of phenomena.

Moreover, unlike Poincaré and many others, Einstein stressed the fact that at stake was not a descriptive theory of electrons or, as he later defined it, a constructive theory like that of Abraham or Lorentz; rather, it was a theory of principle: a theory which stipulates procedures for

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1. Ibid.

2. Poincaré, 1946, pp.305, 507.

3. Ibid., p.308.

4. Indeed, he never elevated the principle of relativity to a convention. (Miller, 1981, p.376 note 7.) However, in Miller's view Kaufmann's results threatened, for Poincaré, not only a theory but also 'a philosophic view..., one which emphasized a principle of relative motion'. (Miller, 1981, p.335.)

attaining physical knowledge.<sup>1</sup> Responding to Ehrenfest's request for more clarification concerning the deductive power of the relativity theory as a closed system of Maxwell-Lorentz electrodynamics, Einstein made it amply clear that he had intended his theory to be basically a theory of principle, and not, as Ehrenfest assumed, a theory of matter.<sup>2</sup> Einstein explained that one should not interpret the two postulates of his theory as a system, let alone as a closed system. Rather, the postulates form according to Einstein a 'heuristic principle which, considered by itself, contains only statements about rigid bodies, clocks and light signals'.<sup>3</sup> In Einstein's view, 'anything beyond that that the theory of relativity supplies is in the connections it requires between laws that would otherwise appear to be independent of one another'.<sup>4</sup>

Having illustrated this consequence of the relativity theory by suggesting how one may enquire into the motion of fast electrons by combining the known laws for slow electrons with the relativistic transformation laws, Einstein concluded that 'we are by no means dealing with a "system" here, a "system" in which the individual laws would implicitly be contained and from which they could be obtained just by deduction'.<sup>5</sup> In Einstein's view, his theory can only furnish 'a principle that allows one to reduce certain laws to others, analogously to the second law of thermodynamics'.<sup>6</sup>

A refutation of the special theory of relativity by an experiment would therefore undermine not a certain conjectured ontology, but a methodology

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1. Einstein, 'What is the Theory of Relativity?', 1981, p.223. Cf., Miller, 1981, p.134.
  2. Einstein, 1950, p.54.
  3. Einstein, 1907,a, p.206 (Ehrenfest's Note is on p.204). Ehrenfest raised the important question as to how does one apply the Lorentz transformations to a rigid body? Cf., Klein, 1967, pp.515-16; Miller, 1981, pp.235-36.
  4. Klein, *ibid.*
  5. Einstein, 1907,a, p.207. Cf., Klein, *ibid.*
  6. Einstein, *ibid.* Klein, *ibid.*

which is purported to give rise to a new knowledge of the physical world. Although Einstein concluded his 1905 relativity paper by deducing from his theory the properties of the motion of the electron which could be accessible to experiments<sup>1</sup> - hence, in principle, refutable - he clearly intended his theory to be situated on some kind of metalevel from which other theories and laws of nature may be looked upon and comprehensively correlated within a broad perspective.<sup>2</sup> Should an experimental refutation of the relativity theory be forthcoming, Einstein, it seems, would have rejected it and required an extended and more profound notion of refutation than just one isolated experiment; Kaufmann's alleged refutation of Einstein's relativity theory is such a case; D.C. Miller's positive result of the 'ether-drift' experiment is another.<sup>3</sup> Indeed, the question arises as to why did Einstein refrain from testing his theory with Kaufmann's available data? A.I. Miller argued convincingly that Einstein had refrained intentionally from doing so; Einstein must have been aware of the fact that his 1905 prediction for

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1. Einstein obtained three relations which constitute, in his words, 'a complete expression for the laws according to which, by the theory here advanced, the electron must move'. (Einstein, 1905, pp.64-65 my emphasis.) The three relationships intended for experimental tests are as follows: (1) the ratio of the magnetic and electric deflection,  $A_m/A_e$ , as a function of  $v/c$ ; (2) the potential difference traversed as a function of  $v$ ; and (3) the radius of curvature of the path of the electron in the presence of a magnetic field as a function of the electron's velocity. (Ibid., p.64.)
  2. However, Einstein did not propose a direct test of his prediction of  $m_t$  for which Kaufmann's results were applicable. As Miller suggested, 'the clue to Einstein's proposal of these three [experimental] tests is that he was searching out "laws according to which... the electron must move", and not merely specialized predictions like those for the electron's mass. For Einstein had convinced himself that the Maxwell-Lorentz electrodynamics was not sufficient for investigating the electron's structure and that theories so based harbored weaknesses'. (Miller, 1981, p.333.)
  3. For references and discussion of D.C. Miller's 'ether-drift' experiment, see Holton, 1975, pp.316-17, 348-50.

the electron's transverse mass,  $m_t$ , disagreed with Kaufmann's data.<sup>1</sup>

Einstein's response to Kaufmann's challenge came in 1907. In a review article, 'On the Principle of Relativity and the Conclusions that Follow from It',<sup>2</sup> under the heading, 'On the Possibility of an Experimental Test of the Theory of Motion of Material Points: The Kaufmann Investigation',<sup>3</sup> Einstein summarized Kaufmann's 1906 paper. In his view, 'W. Kaufmann has determined with admirable care the relation between  $A_m$  and  $A_e$  [the magnetic and electric deflections] for  $\beta$ -rays emitted by a grain of Radium Bromide'.<sup>4</sup> Having explained the theoretical background and the apparatus employed by Kaufmann, Einstein juxtaposed the calculated results based on his theory and the experimental results of Kaufmann in the following graph,<sup>5</sup>

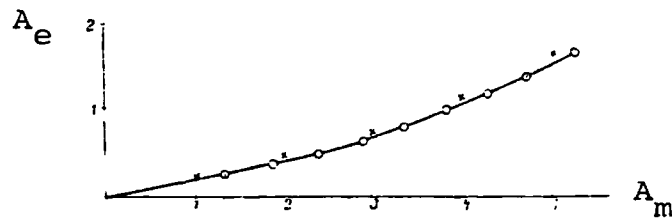


Fig. 2.

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1. Historically, it is quite safe to assume with Miller that when Einstein wrote his relativity paper he was aware of Kaufmann's early experiments. Miller goes on to show that had Einstein used the data of those experiments, he would have obtained values which are about 13% less than those obtained empirically by Kaufmann. This is not a good agreement between theory and experiment. (Miller, 1981, pp.333-34.)
  2. Einstein, 1907,b.
  3. Ibid., p.436.
  4. Ibid., p.437. In Einstein's nomenclature  $A_m$  and  $A_e$  are neither  $z_0$  and  $y_0$  nor  $z'$  and  $y'$ ; however,  $z'$  and  $y'$  demonstrate the relation between  $A_m$  and  $A_e$ . Thus, generally they can be regarded as the magnetic and the electric deflection respectively. For further details see Miller, 1981, pp.344-45.
  5. Einstein, ibid., p.439.

the points and the circles being the observed relations between the electric and magnetic deflections and their respective limits of experimental errors, and the crosses being the calculated values resulting from Einstein's theory.

'In view of the difficulty of the investigation,' Einstein remarked, 'one would be inclined to regard the overall agreement as satisfactory.'<sup>1</sup>

However, as Einstein observed, 'the existing discrepancies are systematic and considerably outside the margin of error of Kaufmann's experiment'.

Yet according to Einstein, 'Kaufmann's calculations are free of error; it follows from the fact that Planck, using a different method of calculation, was led to results which agree entirely with those of Kaufmann'.<sup>2</sup>

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1. Ibid. Einstein pointed out that Kaufmann had not used the observed curve but rather a reduced curve. Einstein made this remark in a footnote and did not elaborate it. (Ibid., p.438.) Miller suggested that this remark could be interpreted as a caveat that was prompted by some inconsistency which Planck had found in Kaufmann's results, namely,  $\beta > 1$ . (Miller, 1981, p.345.) Notwithstanding, Einstein did describe the results as satisfactory (*genügende*). On this inconsistency see Planck, 1906, p.758. Cf., Cushing, 1981, p.1145; Miller, 1981, p.233.
  2. Einstein, *ibid.*, p.439. In his analysis of Planck's criticism of Kaufmann's results, Cushing started on a high note by claiming that 'it was Max Planck who decisively reversed the interpretation of Kaufmann's data from disconfirmation of relativity to confirmation of that theory. Planck's was a classically beautiful application of strict logic to a rather confused situation'. Cushing however ended on a low key with the conclusion that 'even after Planck's reanalysis of Kaufmann's data, the issue was still not clearly decided in favour of either the Abraham or Lorentz theory'. (Cushing, *ibid.*, pp.1143, 1146.) Planck criticized both the apparatus and the interpretation. Thus, two kinds of error were purported to occur in Kaufmann's experiment, namely, error of the second kind: poor vacuum allowed an ionization process to take place which in turn interfered with the electric field; and an error of the fourth kind: the method of reduction was inconsistent, at least in one case. Tackling these two sources of error in one and the same method of analysis is a delicate matter. Moreover, Planck used two different values of  $e/m_0$ . Exercising this two-pronged attack and adopting a new value of  $e/m_0$ , Planck showed that Kaufmann's data favoured what he called the Lorentz-Einstein theory. However, as Cushing himself reports, it was a 'close call (2% vs. 5%) and scarcely overwhelming evidence in favour of relativity. The important outcome of Planck's analysis', Cushing rightly remarks, 'was that Kaufmann's experiments no longer presented a stumbling block to the acceptance of relativity theory'. (Cushing, 1981, p.1146.)

Nevertheless, Einstein refused to draw a definite conclusion from these results. He rather maintained that

whether the systematic discrepancies are due to the existence of some so-far-not-understood source of error or whether they mean that the foundations of Relativity Theory do not correspond with the facts, can surely be decided with certainty only when a great variety of observational material is at hand.<sup>1</sup>

Moreover, he was not prepared to accept either Abraham's theory or that of Bucherer, although, as he himself admitted, the calculated curves they had yielded fitted the observed curve considerably better than the curve obtained from the theory of relativity. In his opinion,

these theories should be ascribed a rather small probability because their basic assumptions about the mass of moving electrons are not made plausible by theoretical systems that encompass a wider complex of phenomena.<sup>2</sup>

Einstein's strong view that his theory transcends the status of a theory of matter and assumes the character of a theory of principle, definitely facilitated his intuitive response that something went wrong in Kaufmann's experiment. His response was indeed intuitive since he could not base his rejection of the results on either experimental or theoretical ground. On the contrary, in his view the measurements were taken with 'admirable care' and the calculations were 'free of error'. Einstein, it emerges, founded his case against Kaufmann's results solely on methodological ground. What perhaps strengthened his methodological argument was the fact that according to the relativity theory the calculated values for the relations between  $A_m$  and  $A_e$  lay in a consistent manner above the observed curve; a fact that could have aroused his suspicion that a systematic error vitiated Kaufmann's results. All the same, a methodological argument - that is, the available theories which could account for Kaufmann's results should nonetheless be ascribed a rather small probability, since, as Einstein argued, their basic postulates had been rendered implausible by a comprehensive

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1. Einstein, *ibid.*

2. *Ibid.* Cf., Pais, 1983, p.159; Pais, 1972, p.82.

theory, namely, the special theory of relativity – sufficed for Einstein to substantiate his intuitive rejection of Kaufmann's results.<sup>1</sup>

This consistent methodological position of Einstein motivated – one is even inclined to say, required – Einstein to suspect an error whose source he could not determine; this suspicion gave in his view sufficient ground for rejecting Kaufmann's experimental results. The contrast between this view and that of Poincaré becomes now clearer; Poincaré had questioned the correctness of a crucial measurement and proceeded to suggest a very plausible source of error; but, as this correct analysis was embedded in a philosophy of science which gave heed to experimental results and dealt with their bearings upon theories of matter, it did not initiate a suspense of judgement, let alone a rejection of the concerned experimental results.

According to Einstein a physical theory can be criticized from two points of view: the degree of its 'external confirmation' and the extent of its 'inner perfection', that is, the degree of its 'logical simplicity'.<sup>2</sup> Here we have been concerned with the former demand which at first sight appears evident: 'the theory,' Einstein stated, 'must not contradict empirical facts.'<sup>3</sup> However, he immediately qualified this demand as he realized that its application had turned out to be quite delicate:

it is often, perhaps even always, possible to adhere to a general theoretical foundation by securing the adaptation of the theory to the facts by means of artificial additional assumptions. In any case, however, this first point of view is concerned with the confirmation of the theoretical foundation by the available empirical facts.<sup>4</sup>

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1. Miller remarked that by 1907 the time was ripe for Einstein to indicate in print his opinion of Kaufmann's data. (Miller, 1981, p.343.) On a more general note Holton commented that 'already in this 1907 article... we have explicit evidence of a hardening of Einstein against the epistemological priority of experiment, not to speak of sensory experience. In the years that followed, Einstein more and more openly put the consistency of a simple and convincing theory... higher in importance than the latest news from the laboratory – and again and again he turned out to be right.' (Holton, 1975, pp.235–36.)
  2. Einstein, 'Autobiographical Notes', 1969, pp.21–23.
  3. Ibid., p.21.
  4. Ibid., pp.21–23. Cf., Miller, 1981, pp.123–25.

Since Einstein's suspicion that there was an error in Kaufmann's experiment and the consequent 'blank' rejection of the experimental result were vindicated, it is surprising that Einstein did not stress or at least suggest in his review of his intellectual experiences the possible occurrences of experimental errors. It is further surprising that he preferred rather to mention the possible adaptability a theoretical system may have for empirical facts, and to make the obvious point that the empirical knowledge which one can have at any historical juncture is limited.<sup>1</sup> Indeed, in contrast to the mellowed remarks of the 'Autobiographical Notes', one can follow Dirac and imagine Einstein responding to Kaufmann's results in the following words: "Well, I have this beautiful theory, and I'm not going to give it up, whatever the experimenters find; let us just wait and see."<sup>2</sup> As it happened, Einstein has so far proved right; 'so it seems', Dirac surmised - confirming thereby his own predilection - 'that one is very well justified in attaching more importance to the beauty of a theory and not allowing oneself to be too much disturbed by experimenters, who might very well be using faulty apparatus'.<sup>3</sup>

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1. Compared to the possible occurrences of experimental errors, these two qualifications are indeed very weak. Referring to his own theory of General Relativity whose great attraction, according to Einstein, is its logical consistency, he remarked that 'if any deduction from it should prove untenable, it must be given up. A modification of it seems impossible without destruction of the whole'. (Einstein, 1950, p.110.) Flexibility and adaptability to empirical facts are not aspects of 'logical simplicity' as Einstein himself indicated here and indeed in his characteristic response to Kaufmann's results: he had refused to let his theory bow, so to speak, to these results.

2. Dirac, 1982, p.83.

3. Ibid.



#### 6.4 Lorentz's Reaction

If Poincaré's and Einstein's reactions to Kaufmann's experimental results constitute the two possible extreme reactions – both suspecting an error, the former however favouring the acceptance of the results, the latter being a categorical rejection – then Lorentz's reaction mediates or rather vacillates between these two poles. As we shall see, Lorentz had accepted the results of Kaufmann's earlier experiments but tried to interpret them within the context of his own 1904 theory. However, in view of Kaufmann's definitive paper of 1906, Lorentz admitted that his theory had been refuted and consequently his contraction hypothesis had to be relinquished. But later on, when other experimental results became available, Lorentz changed his judgement and rejected Kaufmann's results; furthermore, unlike Einstein he took pains to establish the errors which vitiated this experiment.

In 1924 Lorentz remarked that 'one of the lessons which the history of science teaches us is... that we must not too soon be satisfied with what we have achieved. The way of scientific progress,' he observed, 'is not a straight one which we can steadfastly pursue. We are continually seeking our course, now trying one path and then another, many times groping in the dark, and sometimes even retracing our steps.'<sup>1</sup> Although Lorentz illustrated this remark with the way physicists have interpreted the phenomenon of light throughout the ages, the development of his own view with regard to the theory of electrons in general and Kaufmann's experiment in particular also provides such an illustration.

Already in his early scientific work Lorentz had sought to attain a theory of matter which would provide, on the one hand, a consistent ontology, and, on the other, an elucidation of all physical phenomena. This attempt to grasp the laws which govern substance, constituted a persistent motif

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1. Lorentz, 1924, p.608.

throughout Lorentz's scientific work. In his view it is the electromagnetic field - a field which has an independent physical reality - that provides the fundamental substance. By considering the electromagnetic field the state of the ether - which Lorentz identified, when at rest, with absolute space, in contrast to Maxwell, Hertz and others who had considered it the state of the dielectric - Lorentz was able to conceive a new perspective of the foundation of physics.<sup>1</sup>

Lorentz's scientific work in the last decade of the 19th century is characterized by his attempt to furnish equations for the motion of light in macroscopic dielectrics. He was much concerned with explaining several positive effects like the aberration of light, the Doppler effect and the Fizeau experiment (1860).<sup>2</sup> However, his main interest focused upon the negative result of the Michelson-Morley experiment on the motion of the earth through the ether. Lorentz was intrigued by the fact that this experiment - an experiment accurate to second order quantities in the ratio of the earth's velocity to the velocity of light - did not demonstrate the physical consequences of the ether, let alone its very existence as a physical entity. In order to reconcile the experimental null result with his ontology of the ether, Lorentz put forward in 1895 the celebrated contraction hypothesis.<sup>3</sup> However, as he himself conceded in 1904 - when he had already accepted Poincaré's criticism<sup>4</sup> - the method of inventing special hypotheses for each new experimental result was, in his words, 'somewhat artificial'.<sup>5</sup> What is important to note here is not so much the ad-hoc character of Lorentz's early methodology - a methodology which

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1. McCormmach, 1970,b, pp.47ff.; Hirose, 1976, pp.68ff.

2. Miller, 1981, pp.18-29.

3. H.A. Lorentz, 'Michelson's Interference Experiment', in Lorentz et al., 1952, pp.3-7. Cf., Miller, *ibid.*, pp.29-40.

4. Miller, *ibid.*, pp.40-45.

5. Lorentz et al., 1952, p.13. Cf., Holton, 1975, pp.304-16.

he renounced in 1904 - but rather his incessant attempt to obtain a theoretical framework within which all experimental results would be explained. Thus, when J.J. Thomson established experimentally, just before the turn of the century, the existence of charged corpuscles, Lorentz immediately took cognizance of this experimental discovery and shifted his primary concern, as McCormmach observed, 'from the optics of macroscopic bodies to the mechanics of individual electrons'.<sup>1</sup>

The concept of electron furnished Lorentz's theory with a physical entity whose mechanics, when immersed in the ether, could have bridged the gap between the phenomenon of light and that of matter. In Lorentz's view electrons

are extremely small particles, charged with electricity, which are present in immense numbers in all ponderable bodies, and by whose distribution and motions we endeavour to explain all electric and optical phenomena that are not confined to the free ether.<sup>2</sup>

However, the notion of ether remained the fundamental assumption; 'the ether not only occupies all space between molecules, atoms or electrons, but', Lorentz asserted, 'it pervades all these particles'.<sup>3</sup> And whereas the particles may move, 'the ether', Lorentz maintained, 'always remains at rest'.<sup>4</sup>

In Lorentz's theory it is a displacement of a charged particle that produces a change in the state of the ether; this perturbation propagates at the speed of light through the ether and can influence another particle at a later time. These displacements are governed according to Lorentz by electric and magnetic forces. He thus conceived a dynamic theory which is characterized by five equations: the four equations of Maxwell's electromagnetic theory and the Lorentz equation for the forces the ether exerts

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1. McCormmach, 1970,b, p.49.

2. Lorentz, 1952, p.8.

3. Ibid., p.11.

4. Ibid. (emphases in the original.)

on a charged particle:  $\vec{F} = D + (1/c)(\vec{V} \cdot \vec{H})$ . This equation allows for an interaction of field and particle; as Lorentz remarked, it had been obtained by 'generalizing the results of electromagnetic experiments'.<sup>1</sup>

Indeed, Lorentz made explicit this feature of his methodology of close connection between experiment and theory. In his account of the theoretical method he employed, Lorentz observed that the theory of electrons is 'an extension to the domain of electricity of the molecular and atomistic theories that', in his view, 'have proved of so much use in many branches of physics and chemistry'.<sup>2</sup> The method of the molecular and atomistic theories stands in contrast to the method employed by some physicists who, as Lorentz put it,

prefer to push their way into new and unexplored regions by following those great highways of science which we possess in the laws of thermodynamics, or who arrive at important and beautiful results, simply by describing the phenomena and their mutual relations by means of a system of suitable equations.<sup>3</sup>

Lorentz did not deny the success of pure thermodynamics or the achievement of the equations of the electromagnetic field in their most general form; he, however, insisted that by such methods one would have never been able to attain some results to which the molecular hypotheses have given rise.<sup>4</sup> The fruitfulness of these hypotheses cannot be denied, Lorentz claimed, 'by those who have followed the splendid researches on the conduction of electricity through gases of J.J. Thomson and his fellow workers'.<sup>5</sup> Lorentz, as a theoretician who stood, so to speak, on experimental ground, clearly saw himself as one of those who followed closely the new experimental developments at the turn of the century.

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1. Ibid., p.14.

2. Ibid., p.10.

3. Ibid.

4. Recalling Einstein's distinction between a constructive theory and a theory of principle, we can clearly see that Lorentz's theory is constructive.

5. Lorentz, op. cit., p.11.

As Holton observed, Lorentz at that time was

deeply involved in constructing, step by step, a viable theory for electrodynamics, based as far as possible on existing principles and mechanisms, relying on experimental results as a guide to the detailed construction of a modification of existing theory.<sup>1</sup>

And indeed Lorentz's important paper of 1904, 'Electromagnetic Phenomena in a System Moving with any Velocity less than that of Light',<sup>2</sup> contains, in addition to a new theory of electrons, many experimental considerations. Lorentz in fact remarked that both experimental and methodological reasons had led him to re-examine the problems which are connected with the motion of the Earth. Specifically, the two main reasons were the new experiments of Michelson, Rayleigh, Brace, and Trouton and Noble<sup>3</sup> - experiments in which quantities of the order  $v^2/c^2$  could have been perceptible, though the results were negative - and, secondly, the quest for fundamental assumptions which would dispense with ad-hoc hypotheses.<sup>4</sup>

Lorentz's theory incorporates, as has been observed, the four electromagnetic equations of Maxwell and the dynamic equation of Lorentz; it further assumes a privileged frame of reference at rest in the ether that constitutes, therefore, absolute space and time in the Newtonian sense. In this privileged frame all rods are of maximum length and time goes fastest in comparison to rods that contract and 'local times' that dilate in reference frames that move with respect to this privileged frame.<sup>5</sup> Having constructed his theory - a theory which, unlike that of Einstein, presupposes the classical principle of relativity - Lorentz proceeded to show how it could account for a large number of experimental facts. He explained the negative results of the afore-mentioned experiments and concluded his paper, quite appropriately,

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1. Holton, 1975, p.306.

2. Lorentz et al., 1952, pp.11-34.

3. For the references see *ibid.*, pp.3, 4 and 11.

4. *Ibid.*, pp.11-13.

5. Whitrow, 1973, pp.108-9; Zahar, 1973, pp.121-22.

with an analysis of Kaufmann's 1902 and 1903 results. He thus attempted to demonstrate how his theory of electrons can explain a whole range of experiments on the basis of some fundamental mechanism that lies, as Lorentz put it, 'at the bottom of the phenomena'.<sup>1</sup>

In this important paper of 1904, Lorentz accepted Kaufmann's results and considered them decisive; that is, he did not question their validity but rather demonstrated how his theory could account for the results: the formula for the transverse mass being in accordance with the data at least as well as that of Abraham. According to Lorentz, Abraham's theoretical results were 'confirmed in a most remarkable way by Kaufmann's measurements of the deflection of radium-rays in electric and magnetic fields. Therefore', Lorentz argued, 'if there is not to be a most serious objection to the theory I have now proposed, it must be possible to show that those measurements agree with my values nearly as well as with those of Abraham'.<sup>2</sup> Lorentz, it appears, did not even contemplate, at least in this paper, the possibility that Kaufmann's experiment might be wrong; moreover, he mentioned neither the small difference between the predictions of his theory and that of Abraham nor the large inaccuracy in Kaufmann's measurements reported by Abraham in 1903.<sup>3</sup> For Lorentz, apparently, it was sufficient that a satisfactory agreement had been attained between the experimental results and his theory; he simply did not take the trouble to analyse the experiment itself, and accepted its results, as he had accepted other experimental results, to serve as a test and a guide to his theory.

Lorentz's reaction to Kaufmann's early results amounts in effect to the claim that there might have occurred an error of interpretation: Lorentz analysed the numerical results of Kaufmann's measurements within

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1. Lorentz, 1952, p.8.

2. Lorentz et al., 1952, p.31.

3. Supra, p. 300.

his own theory, and showed how a satisfactory agreement with his own formulae could be obtained.<sup>1</sup>

As a result of Lorentz's new interpretation, Kaufmann's experiment lost its conclusiveness. Kaufmann therefore re-did the experiment in an attempt to force a decision primarily between the theory of Abraham and that of Lorentz.<sup>2</sup> Although the new measurements did not vindicate conclusively Abraham's theory, they definitely refuted, in Kaufmann's view, Lorentz's theory.

In the spring of 1906 Lorentz delivered in Columbia University a series of lectures on the subject of the theory of electrons. The lectures were published in 1909 and have since then - with the additional footnotes in the second edition of 1915 - served as an outstanding exemplar of physical thinking which combines general principles and experimental results. From these lectures it transpires that in 1906 Lorentz admitted that, unlike his general physical credo which Kaufmann's results had confirmed, his own particular theory of electrons had in fact been refuted by these very results. It is worthwhile to quote Lorentz's overview at some length:

Of late the question has been much discussed, as to whether the idea that there is no material but only electromagnetic mass, which in the case of negative electrons, is so strongly supported by Kaufmann's results, may not be extended to positive electrons [*sic*] and to matter in general... What we really want to know is, whether the mass

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1. Essentially, Lorentz replaced Abraham's formulae for  $m_t$  and  $\Phi(\beta)$  with his own formulae. He first showed that one constant in Kaufmann's data analysis remained satisfactorily constant under this change of formulae; then using Kaufmann's measured values of  $z'$  he deduced the corresponding values of  $y'$  with which he could compare the values of Kaufmann's results. For details see Lorentz, *op.cit.*, pp.31-34; Miller, 1981, pp.74-75; Cushing, 1981, pp.1142-43.
  2. Miller reports a letter Kaufmann wrote to Lorentz in July, 1904. In it Kaufmann admitted that with the data at hand it was difficult to decide between the two theories. He was however of the opinion that Lorentz's prediction agreed better. In any event he promised, 'on account of the great importance of the whole problem', to repeat the measurements with increased accuracy. (Miller, *ibid.*, p.75.)

of the positive electron can be calculated from the distribution of its charge in the same way as we can determine the mass of a negative particle. This remains, I believe, an open question, about which we shall do well to speak with some reserve.

In a more general sense, I for one should be quite willing to adopt an electromagnetic theory of matter and of the forces between material particles. As regards matter, many arguments point to the conclusion that its ultimate particles always carry electric charges and that these are not merely accessory but very essential. We should introduce what seems to me an unnecessary dualism, if we considered these charges and what else there may be in the particles as wholly distinct from each other.

On the other hand, I believe every physicist feels inclined to the view that all the forces exerted by one particle on another, all molecular actions and gravity itself, are transmitted in some way by the ether, so that the tension of a stretched rope and the elasticity of an iron bar must find their explanation in what goes on in the ether between the molecules. Therefore, since we can hardly admit that one and the same medium is capable of transmitting two or more actions by wholly different mechanisms, all forces may be regarded as connected more or less intimately with those which we study in electromagnetism.

For the present, however, the nature of this connection is entirely unknown to us...

Clearly, Lorentz believed in an electromagnetic synthesis in which ether and charged particles are the fundamental concepts; matter, in its usual sense, is just superfluous. In that respect, Kaufmann's results encouraged Lorentz to think that he was on the right track. And indeed Lorentz cited Kaufmann's 1906 experiment with approval arguing that 'it will be best to admit Kaufmann's conclusion, or hypothesis, if we prefer so to call it, that the negative electrons have no material mass at all'.<sup>2</sup> In Lorentz's view this conclusion 'is certainly one of the most important results of modern physics'.<sup>3</sup>

However, Lorentz was aware that the quantitative conclusion of Kaufmann had undermined his theory which could account, on the basis of the contraction hypothesis, for the negative results of second order experiments.

'So far as we can judge at present,' Lorentz observed, 'the facts are against our hypothesis... Kaufmann,' Lorentz continued, 'has repeated his experiments with the utmost care and for the express purpose of testing my assumption. His new numbers agree within the

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1. Lorentz, 1952, pp.45-46.

3. Ibid.

2. Ibid., p.43.



limits of experimental errors with the formulae given by Abraham, but not so with... [my equation for the transverse mass], so that,' Lorentz concluded, 'they are decidedly unfavourable to the idea of a contraction, such as I attempted to work out.'<sup>1</sup>

It thus seemed to Lorentz that he had to relinquish the idea of a contracting electron altogether.

In March 1906, Lorentz confessed in a letter to Poincaré that

unfortunately my hypothesis of the flattening of electrons is in contradiction with Kaufmann's new results, and I must abandon it. I am, therefore, at the end of my Latin. It seems to me impossible to establish a theory that demands the complete absence of<sup>2</sup> an influence of translation on the phenomena of electricity and optics.

Notwithstanding this conclusion, Lorentz considered it legitimate to maintain his refuted hypothesis and to pursue its implications, as long as some progress in the understanding of physical phenomena could be made by employing it. He stressed that in speculating on the structure of the electron it should not be forgotten that

there may be many possibilities not dreamt of at present; it may very well be that other internal forces serve to ensure the stability of the system, and perhaps, after all, we are wholly on the wrong track when we apply to the parts of an electron our ordinary notion of force.<sup>3</sup>

It is noteworthy that even when Lorentz tried to secure a viable position for his hypothesis, he did not resort to the possibility that Kaufmann's experiment might be erroneous; rather, he transformed the idea of limited knowledge into an argument which was supposed to substantiate the legitimacy of his refuted theory.

In the preface to the first edition of the Theory of Electrons, Lorentz noted that he had not treated Einstein's principle of relativity adequately. Indeed, Einstein's theory of special relativity received only a sketchy account at the end of the book. According to Lorentz, Einstein's results generally agreed with those that he had obtained; in Lorentz's view, the main difference was that Einstein 'simply postulates' what he, that is,

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1. Ibid., pp.212-13.

3. Lorentz, 1952, p.215.

2. Quoted by Miller, 1981, p.334.

Lorentz, claimed to 'have deduced, with some difficulty and not altogether satisfactorily, from the fundamental equations of the electromagnetic field'.<sup>1</sup> Lorentz remarked further that Einstein's results had led to the same contradiction with Kaufmann's experiment as his own results had done.<sup>2</sup> Lorentz thus appeared to grasp quite clearly - unlike, for example, Kaufmann and Planck - that although the two theories agree formally, they differ in their underlying principles.<sup>3</sup>

In the light of these views, it can be said with some force that Lorentz was not prepared, from a methodological point of view, to doubt, let alone reject, Kaufmann's results. In 1904 he demonstrated to his own satisfaction how his theory could account for Kaufmann's 1902 and 1903 results; however, in 1906, when Kaufmann published his definitive paper, Lorentz felt obliged to uphold its results and, as a consequence, to relinquish his contraction hypothesis which stood at the centre of his theory. Qualitatively Kaufmann's results enhanced Lorentz's general conception of an electromagnetic synthesis; however, they undermined, from a quantitative point of view, his particular theory of electrons. Lorentz, it seems, was caught in a dilemma: on the one hand, had he accepted Kaufmann's results his general view of the physical world, unlike his theory of electrons, would have been substantiated, and, on the other hand, had he rejected the experimental results he would have perhaps saved his theory of electrons but then jeopardized his general view. Lorentz's philosophy of science which considers experiment a guide for developing theories, constrained him, I suggest, to choose the former

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1. Ibid., p.230.

2. Ibid., pp.229-30.

3. Zahar observed that 'before the advent of General Relativity the scientific community (e.g. Planck, Poincaré, Bucherer, Kaufmann and Ritz) spoke of the Lorentz-Einstein theory and contrasted it with the more classical theories of Abraham and Ritz: they regarded the theories of Lorentz and Einstein as observationally equivalent'. (Zahar, 1973, p.259.) On Planck's misunderstanding see, Miller, 1981, pp.254, 256.

horn of the dilemma. He simply did not contemplate, or hint at the middle way, let alone the latter horn. For Lorentz to reject an experiment, that is, to render it erroneous, would require new experimental results which contradict it conclusively with no room for doubts. Such experimental results were forthcoming in 1909 when the first edition of the Theory of Electrons was published, and between 1909 and 1915 it became abundantly clear that Kaufmann's experiment was indeed erroneous. The footnotes of the second edition of the Theory of Electrons bear witness to the change that took place in Lorentz's view of Kaufmann's experiment.

Whereas in 1909 Lorentz had admitted that his theory had been refuted by Kaufmann's experiment and that the contraction hypothesis had had to be relinquished, in 1915 he noted that 'this can no longer be said now'.<sup>1</sup> He referred the reader to the experiments of Bucherer, Hupka, Schaefer and Neumann, and Guye and Lavanchy which, in his opinion, confirmed his formula for the transverse electromagnetic mass. He therefore claimed that 'in all probability, the only objection that could be raised against the hypothesis of the deformable electron and the principle of relativity has now been removed'.<sup>2</sup> It is noteworthy that Lorentz mentioned both the hypothesis of the deformable electron and the principle of relativity; although one can deduce within the framework of Einstein's theory the contraction phenomenon, Lorentz seemed to consider here two separate theories.<sup>3</sup>

It is significant that in the section where Lorentz deals with the qualitative aspect of Kaufmann's experiment, prior to the discussion of its quantitative results, he does not inform the reader that this experiment

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1. Lorentz, 1952, p.212 footnote 1.

2. Ibid., p.339.

3. Pais noted that 'as late as 1909 Poincaré did not know that the contraction of rods is a consequence of the two Einstein postulates. Poincaré therefore did not understand one of the most basic traits of special relativity'. (Pais, 1983, p.168.)

was decisively contradicted.<sup>1</sup> Since the phenomenon that the mass of a charged particle depends upon its velocity had been confirmed by the aforementioned experiments<sup>2</sup> and, furthermore, since Kaufmann's conclusion that the rest mass of electron can be regarded as zero had not been contradicted by these experiments, Lorentz presumably felt entitled to adhere to his 1909 account and did not amend this section. Thus, in the light of the new experimental results Lorentz was able to resolve his dilemma. He could now reject the quantitative results of Kaufmann's experiment which confirmed Abraham's formula for the transverse electromagnetic mass, and accept its qualitative result which enhanced his idea of an electromagnetic synthesis.

However, in 1915 it was not only an experiment that challenged Lorentz's theory but also a theory, namely, Einstein's theory; a theory which was beginning by then to gain wide recognition. Having rejected the experimental refutation of his theory, Lorentz now faced the simplicity and great heuristic power of Einstein's theory. He admitted that if he were to write the account of Einstein's theory again, he would have certainly given it a more prominent place.<sup>3</sup> And he remarked that with Einstein's theory 'the theory of electromagnetic phenomena in moving systems gains a simplicity that [he]... had not been able to attain'.<sup>4</sup> As Lorentz viewed it, the main cause of his failure was his adherence to the idea that only the time  $t$  in the privileged rest frame could be considered as the true time; the 'local time'  $t'$  being no more than an auxiliary mathematical quantity. The simplicity of Einstein's theory lies of course in its stipulation that  $t'$  carries the same physical significance as  $t$ . 'If we want to describe phenomena in terms of  $x', y', z', t'$ , we must work,' Lorentz concluded, 'with these variables exactly as

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1. Lorentz, 1952, pp.42-43.

2. The experiments of Bucherer, Hupka, Schaefer and Neumann, and Guye and Lavanchy. For references see Lorentz, *ibid.*, p.339.

3. *Ibid.*, p.321.

4. *Ibid.*

we could do with  $x, y, z, t$ .<sup>1</sup>

Since Lorentz's formulae agree formally with those of Einstein, the realization that Kaufmann's experiment was erroneous supported not only Lorentz's theory but that of Einstein as well. Lorentz was careful not to omit this important point and he refers the reader - where he remarks that Einstein's results contradicted Kaufmann's results - to the same footnote to which he previously alluded when he dealt with his own results.<sup>2</sup> It is therefore no wonder that in this footnote Lorentz distinguishes between the hypothesis of the deformable electron and the principle of relativity, referring, no doubt, to his theory and to that of Einstein respectively.

In view of the fact that in 1915 Lorentz left intact his 1909 concluding remarks, we may surmise that Lorentz did not budge from his theoretical standpoint and steadfastly held to his view of the ether as the fundamental concept. He wrote,

I think something may also be claimed in favour of the form in which I have presented the theory. I cannot but regard the ether, which can be the seat of an electromagnetic field with its energy and its vibrations, as endowed with a certain degree of substantiality, however different it may be from all ordinary matter.<sup>3</sup>

Although Lorentz did recognize the importance of Einstein's theory, he was of the opinion that 'each physicist can adopt the attitude which best accords with the way of thinking to which he is accustomed'.<sup>4</sup> Lorentz's adherence to a classical ontology in which the ultimate constituents are the charged corpuscle and the ether - absolute space and absolute time - was neither wrong-headed nor idiosyncratic; on the contrary, as Zahar convincingly argued, there was a subtle rationale behind Lorentz's obstinacy.<sup>5</sup>

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1. Ibid. Cf., Zahar, 1973, pp.119-20.

2. Lorentz, *ibid.*, pp.213,230,339.

3. Ibid., p.230.

4. Quoted by Hirose, 1976, p.70. Cf., Lorentz, 1931, p.210.

5. Zahar, 1973, pp.121-23.

It is however surprising to find Zahar remarking that by 1915 Lorentz had already accepted the Relativity Principle.<sup>1</sup> It seemed plausible to Zahar that 'Lorentz was converted to Relativity by the realization that, while covariance was opening up new possibilities, the ether had become heuristically sterile'.<sup>2</sup> Zahar indeed conjectured that 'had Lorentz known right from the beginning that Kaufmann's experiment was not crucial, he would most probably have accepted the covariance of Maxwell's equations and joined the Relativity Programme at its inception in 1905'.<sup>3</sup> However, in view of the fact that Lorentz carefully distinguished in 1909 as well as in 1915 between his theory and that of Einstein, and alluded in both cases to the new experimental results, it seems doubtful that Zahar's conjecture is correct. As Born pointed out, Lorentz had never wholly accepted Einstein's theory and seemed to remain in doubt as to its physical perspective.<sup>4</sup>

At all events, for Lorentz the validity or otherwise of the principle of relativity was a question of particular properties of the natural forces and therefore should be decided by experiment and not by theory. 'It must be possible to decide experimentally,' Lorentz maintained, 'whether the principle of relativity holds or does not hold.'<sup>5</sup> Thus, unlike Einstein who raised this principle to the status of a postulate, Lorentz was looking for its experimental manifestation. It is therefore natural to find Lorentz analysing the experimental evidence and determining, as late as 1922, the errors that vitiated Kaufmann's experiment.

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1. Ibid., p.238.

2. Zahar, 1978, p.80.

3. Ibid.

4. Born, 1956, p.192. Elsewhere he wrote that Lorentz 'probably never became a relativist at all, and only paid lip service to Einstein at times in order to avoid argument'. (Born, 1971, p.198.) In Pais' view, 'Lorentz never fully made the transition from the old dynamics to the new kinematics'. (Pais, 1983, p.167.) Cf., Lorentz, 1931, pp.208-11.

5. Lorentz, *ibid.*, pp.255, 266.

In Lorentz's view the investigation of the motion of free electrons affords the best chance for deciding whether or not the principle of relativity holds. He noted that only when the velocities of the free electrons are comparable with that of light is there an appreciable difference between the opposing theories of spherical and flattened electrons.<sup>1</sup> However, Lorentz realized that in an experiment such as that of Kaufmann,

too great velocities... have to be avoided; ...[since] the masses, which for  $\beta$  approaching 1 tend to infinity, would then be too great, and the accelerations, and thus also the deflections produced<sub>2</sub> by the applied fields, would be too small to be accurately measured.

Lorentz pointed out further that the glass vessel within which the electrons travel in the set-up of Kaufmann's experiment

must be well evacuated not only to avoid collisions of the  $\beta$  -particles with the air molecules, which would give diffuse deflections, but also to prevent the formation of a current between the plates<sub>3</sub> of the condenser, which would spoil the uniformity of the field.

Thus, as late as 1922, Lorentz thought it important to acquaint the reader with the technical difficulties of Kaufmann's experiment; an experiment whose line of investigation Lorentz considered correct and viable.

Having described the experiment itself Lorentz reported that Kaufmann 'believed himself to be driven to the conclusion that the theory of the spherical electron fitted better'.<sup>4</sup> However, by 1922 Lorentz had already been in a position to refute Kaufmann's claim; namely, there were available abundant experimental results which contradicted this claim. With the backing, so to speak, of these new results, Lorentz searched for the errors in Kaufmann's experiment. He remarked in the first place that on the basis of the spherical electron theory there were great deviations between the calculated and the measured values; deviations which were, as he put it, 'left behind'. Moreover, Lorentz continued,

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1. See supra, p.305.

3. Ibid., p.272.

2. Lorentz, 1931, p.267.

4. Ibid., p.274.

a number of sources of experimental error can be pointed out. Thus, e.g., the vacuum was not high enough. In fact, now and then a spark passed between the plates of the condenser, which shows that there was always some ionisation current left between these plates, and that therefore the homogeneity of the electric field was not above doubt. In fine, no definite verdict can be based upon Kaufmann's experiments in favour of either theory.<sup>1</sup>

Thus Lorentz maintained that in fact Kaufmann's experiment had not confirmed Abraham's theory of the spherical electron, even if one were to consider the experiment correct; but then it was rendered erroneous particularly because of the false assumption that the electric field was homogeneous.<sup>2</sup>

From the standpoint of the classification I have proposed, we may observe here a shift in Lorentz's view. Whereas in 1904 he had suggested that Kaufmann's result could be interpreted differently - implying thereby that an error of interpretation might have occurred - in 1922 he maintained that Kaufmann had made mistakes in his analysis of the data and, more importantly, assumed erroneously that the electric field was homogeneous. Thus, at one time Kaufmann's error is characterized as pertaining to the class

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1. Ibid. Lorentz does not report the sources of these evidences. The implication of these remarks is that it was possible to establish the sources of error in Kaufmann's experiment solely on the basis of a thorough examination of the experiment itself.
  2. Cohen, Crowe and DuMond reported that A.E. Shaw had made an important discovery concerning the experimental technique of applying an electric field in vacuum. His discovery 'revealed a source of previously unsuspected systematic error in all such work. It was shown that in vacuum there are formed, on or very close to the metal plates between which electric fields are established, polarization charges which effectively reduce the potential used to accelerate or deflect an electron beam. Furthermore, a charge seems to accumulate on a more or less permanent insulating layer which forms on metal surfaces in vacuum when these are bombarded with electrons. Such effects depend on the material of the metal plates, the residual gas pressure, the cleanliness of the vacuum pumping arrangements (freedom from organic materials), and the intensity of the electron bombardment. Residual gas such as oxygen adhering to the metal may account for the effect in part. It has been found that vaporizing a layer of gold on the interior of the entire vacuum chamber so as to cover the surfaces of all metal parts reduces these effects materially. However, the lesson from Shaw's work is one which every experimental physicist should remember: it is almost impossible to hope to define the potential of an evacuated region by means of electrically conducting metal walls, slits, or what-not with an uncertainty very much smaller than  $\pm 1$  volt. Many an otherwise well-planned experiment has ended in disappointment because of ignorance of this difficulty'. (Cohen, Crowe and DuMond, 1957, pp.139, 181.)



of 'theoretical conclusions', and at another time as that of 'assumptions concerning the actual set-up and its working'. We may note further that the transition took place in two phases: in 1909 the experimental result was accepted and, in 1915, it was rejected with no explanation, merely on the basis of counter experimental results.

Since the 1922 objections to Kaufmann's experimental results are independent of any knowledge of other experimental results, and, furthermore, in view of the fact that Lorentz considered Kaufmann's experiment crucial with regard to the validity or otherwise of his own theory, it is difficult to understand why Lorentz did not subject Kaufmann's definitive paper of 1906 to a thorough and rigorous analysis, but rather conceded that Kaufmann had shown that the contraction hypothesis was untenable.<sup>1</sup> In the light of this historical account we may suggest that only when Lorentz was fortified with a battery of experiments which had confirmed his theory and therefore contradicted Kaufmann's results, did he examine afresh the experiment at issue and eventually pronounce it inconclusive and indeed erroneous. Had it not been for the opposing results of many experiments, Lorentz, it seems, would not have attempted a critical re-examination of Kaufmann's experiment. However, it should be stressed that the experiments which Lorentz cited had not provided him with any critical analysis of Kaufmann's experiment; though they investigated, like Kaufmann's experiment, the motion of free electrons, they sought theoretically and experimentally different types of demonstration. These experiments simply did not constitute in any way re-runs of Kaufmann's experiment.

For Lorentz - an eminent theoretician who considered experiment both a test and a guide for theory - to discard experimental results, that is,

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1. For comparison, Planck suspected right from the time of Kaufmann's 1905 and 1906 publications that the experiment was erroneous. As a protagonist of the theory of relativity, Planck subjected Kaufmann's experiment to both logical and physical analyses, and endeavoured to establish its errors already in 1906. (Supra, p.328 footnote no.2; Planck, 1906, 1907. Cf., Zahar, 1973, pp.238 footnote no.1, 241-42; Pais, 1983, pp.150-51.)

to pronounce an experiment erroneous, would require the knowledge of several other experiments which did not conform to its results. In other words, the rejection of experimental results requires, at least in the case of Lorentz, the attainment of a vantage point from whence other experimental results can be seen to replace the results in question.<sup>1</sup> A philosophy of science of the kind Lorentz held which sees in experiment not only a means of testing the validity of a theory but also a guide that may indicate further developments, simply cannot sustain - unlike, for example, Einstein's philosophy of science - the 'vacuum' of knowledge which a rejected experiment may leave behind it if there were to be no other experimental results to replace it. Furthermore, it is a contradiction between experimental results that motivates a scientist who holds such a philosophy of science to critically re-examine the experiments at issue; an examination which in Lorentz's case eventually revealed errors of the second kind.<sup>2</sup>

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1. Holton also appears to hold this view. Remarking that ten years had had to elapse before it was fully realized - through the experimental work of Guye and Lavanchy in 1916 - that 'Kaufmann's equipment was inadequate [sic]', Holton implied precisely this point. (Holton, 1975, p.235.)
  2. Born's historical sketch of the role Kaufmann's experiments played cannot be sustained in view of the above historical analysis. According to Born 'experiments by Kaufmann (1901) [sic] and others who have deflected cathode rays [sic] by electric and magnetic fields have shown very accurately [sic] that the mass of electrons grows with velocity according to Lorentz's formula  $m(v) = m_0/(1-v^2/c^2)^{1/2}$ '. On the other hand, these measurements can no longer be regarded as a confirmation of the assumption that all mass is of electromagnetic origin. For Einstein's theory of relativity shows that mass as such, regardless of its origin, must depend on velocity in the way described by Lorentz's formula'. (Born, 1965, p.278.) Notice that Born implied that Kaufmann's experiments contained an error of the fourth kind: an error of interpretation.

## Conclusion

The polarity between the claim to knowledge as certainty and the inherent fallibility of the human faculties which are supposed to attain knowledge, points to the problem of error. By acknowledging man's potential fallibility one would be inclined to consider the claim to knowledge uncertain. If one were to persist in one's claim to knowledge as certainty, one would then be required - having admitted fallibility - to address oneself to the problem of error and to examine error as an obstacle which obstructs the attainment of knowledge.

Indeed, as has been shown in the thesis, past philosophers of grand systems were required by the very systems which they conceived, to enter into an analysis of the problem of error. Once they determined - in the light of the philosophy each of them propounded - the nature of error, they sought methods which they believed could clear the way to knowledge. However, their ultimate goal remained knowledge as certainty; thus, no room for errors was left in these grand designs. For philosophers like Bacon, Descartes and Spinoza, errors could and should be eliminated with, as Bacon puts it, 'a constant and solemn determination, and the Intellect entirely freed and purged from them, so that the approach to the Kingdom of Man... may be like that to the Kingdom of Heaven'.<sup>1</sup>

However, as the case of Kaufmann's experiment and the varied responses which it received, confirms, the approach to the 'Kingdom of Man' is fraught with persistent difficulties. The Kaufmann case-study highlights amongst other things the difficulty which Wittgenstein pointed out, namely, that giving the assurance 'I know' does not suffice. It is after all only a claim that I have not committed an error. 'I know that' means 'I am

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1. Bacon, 1859, p.49 (Bk.I, lxviii). The rest of the quotation reads: 'into which none may enter save in the character of a little child'. (A reference to Matthew, xviii, 3.)

incapable of being wrong about that'; but whether this is indeed the case would have to admit of being established objectively. That however cannot be done. As this case-study shows, the 'I' remains essential and that cannot lead to objective certainty, to the 'Kingdom of Heaven'.

Experimental error - an error which arises in the method of experimentation - obstructs this attempt to attain categorical objectivity, and hence knowledge of the physical world. Indeed, it appears that it is partly due to experimental errors that one often finds divergence of opinion on matters which are amenable to experiments. The attempt to provide an account of the concept of experimental error may be seen as a project which questions the conclusiveness of experimental results by exploring the theoretical and practical difficulties which attend the execution of an experiment. However, whilst I stress the importance of acknowledging the concept of experimental error, I do not seek to rule out altogether knowledge based on experiments. On the contrary, I consider the method of experimentation a viable means of study which has transformed man's knowledge of the physical world. In acknowledging the importance of the concept of experimental error, I wish to introduce an element of caution into one's belief in experimental results. I thus take seriously the Socratic view that having explored the shortcomings of one's claim to knowledge, one would investigate better and be too modest to fancy that one knows what one does not know.<sup>1</sup> However, I am not only concerned with this negative aspect of the concept of experimental error. I also draw attention to its positive aspect: the comprehension of an error, its discovery and assessment, may contribute to knowledge.

I study the concept of experimental error in two distinct settings. In the first part of the thesis I attempted to relate - within an historical setting - the belief in unity and physical realism to the emergence of a novel understanding of this concept. In the second part - the analytical

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1. Plato, 1949, p.84 (210).

setting - I endeavoured to chart systematically possible pitfalls of experiment and illustrate them.

In the historical setting I introduced some views from antiquity which show that the problem of experimental error was not then fully understood and at best only implicitly formed. I then contrasted this result with Kepler's explicit and comprehensive view of the concept concerned. As a study of the Astronomia Nova shows, Kepler exploited his awareness of the occurrences of experimental errors to guide him to the right conclusion. Thus errors were employed, so to speak, perhaps for the first time, to bring about a major discovery. 'Know then,' to use Kepler's own words, 'that errors show us the way to truth.'<sup>1</sup> With a survey of Kepler's revolutionary contribution to optics, I demonstrated that Kepler's view of the problem of experimental error extended beyond mere discrepancies between calculations and observations to other types of error, especially to those of the observational kind. It emerged that Kepler's belief in unity and physical realism facilitated, indeed created, the right philosophical posture for comprehending the problem of error in an entirely novel way.

In attempting to chart pitfalls which may occur in experiments, I followed the scholastic maxim that one never really knows what a thing is unless one is also able to give a sufficient account of its opposite. In works of logic it is commonly found that a section is devoted to the description and classification of the different ways in which the rules of logic may be transgressed. As J.S. Mill puts it, 'the philosophy of reasoning to be complete, ought to comprise the theory of bad as well as of good reasoning'.<sup>2</sup> In a somewhat similar fashion, I described and

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1. Quoted by Koyré, 1973, p.250. Kepler's words are: 'Scito itaque, quod hi errores via nobis futuri sint ad veritatem.' (Kepler, 1938-1975, III pp.313-14.) Bacon, a contemporary of Kepler, expressed it from a different angle. 'Truth,' he wrote, 'emerges more quickly from error than from confusion.' (Bacon, 1859, p.183 (Bk.II, xx).)

2. Mill, 1843, p.333.

classified in the analytical section of the thesis the ways in which an experiment may go wrong. However, I pointed out that a classification of experimental errors can never be, in principle, exhaustive. Yet, by choosing coherent and fruitful criteria, it may be possible to obtain a classification which is, on the one hand, comprehensive and, on the other, clear in its detailed distinctions.

I proposed a classification which consists of four categories. They are: 1. Background theory; 2. Assumptions concerning the actual set-up and its working; 3. Observational reports; and 4. Theoretical conclusions. These four classes correspond to the four distinct stages which can be discerned in the execution of an experiment, namely, 1. Laying down the theoretical framework of the experiment; 2. Constructing the apparatus and making it work; 3. Taking observations or readings; and 4. Processing the recorded data and interpreting them. As the headings of the classes suggest, I was concerned to break down what may be called the experiment's argument into its different components.

The background theory provides the theoretical underpinnings of the experiment. This theory is not under test; to avoid confusion it is crucial to separate it from the theoretical conclusions. The inappropriate use of Stokes' law which F. Ehrenhaft made in his measurements of the charge of the electron illustrates an error which arises in this first class.

The second class, the class of the assumptions concerning the actual set-up and its working, includes considerations of the set-up as an independent physical system, its parts and their working to the specified design. H. Hertz's experiment on the deflection of cathode rays in an electric field - a deflection which he failed to record - provides an illustration of an error of the second kind.

In the third class I focused on the process of observation, attempting to isolate difficulties which may arise in observing a phenomenon or in taking readings. In the N-ray case, R. Blondlot, who had believed himself

to be the discoverer of a new form of radiation, stipulated that the effects of this so-called radiation could be seen only if one were to examine the screen upon which the rays had been projected as an impressionist painter would look at landscape. The subjective element is therefore of paramount importance in the discussion of this class.

The final stage of an experiment is the stage of comprehending the data. In this class I pointed to possible pitfalls in the process of reducing the data, assessing and interpreting them. The case of J. Franck's and G. Hertz's experiment on the quantized spectrum of the atom's energy levels illustrates a correct measurement whose interpretation was wrong: the critical potential they had measured was not an ionization potential as they thought, but rather an excitation potential.

This classification throws light not only on the different ways in which an experiment may turn out wrong, but also on the epistemological differences in discovering that an experiment is in error. Epistemologically, there is a difference in going wrong within the scope of the first three categories as distinct from the fourth one. In the first three categories the experimenter can, at least in principle, spell out all the assumptions on which the result is predicated; this is not the case with respect to interpreting the data. In assigning a physical meaning to an experimental result, the experimenter, or for that matter the theoretician, either draws upon existing theories or calls for a new one; in either case he must exercise a judgement which cannot be anchored to assumptions but to insights. The 'I' yet again remains essential.

Finally, several problems arise in view of this thesis. First and foremost there is the problem of relating the mathematical theory of error to the present broad treatment of the concept of error. Then there is the project of following historically the development of the concept of experimental error in the sense I have here propounded, and the emergence of related concepts such as accuracy and reliability. From the philosophical

point of view, the occurrence of error and its realization may enhance the belief in physical realism. Error appears to play a dual role: its occurrence underlines the fallibility of the subjective 'I', but at the same time, by its very occurrence, it indicates the possibility of an objective physical reality to exist. I suggest that the distinction between error and mistake can be useful for such an investigation which, together with a classification of types of experimental error, may contribute to an epistemology of experiment. All in all, experimental error - a much neglected concept in the annals of philosophy of science - ought to receive more consideration if philosophy of science is to pose not only problems of theoretical reconstructions and the related problem of rationality, but also problems that arise in the actual practice of the scientific method.



## A Classification of Types of Experimental Error

Categories	Background Theory	Assumptions Concerning the Actual Set-up and its Working	Observational Reports	Theoretical Conclusions
The stages of an experiment	Laying down the theoretical framework	Constructing the apparatus and making it work	Taking observations or readings	Processing the recorded data and interpreting them
Possible sources of mistake and error	1. False general theory 2. False instrumental theories 3. False theory of the set-up	1. Parts which are not up to the standard 2. The dirt factor: impure samples; poor vacuum; convection current; thermal expansion; diffusion 3. Incorrect calibration; faulty graduation of a scale; pointer is not pivoted at the centre	1. Observational error: parallax; aberration; refraction; effective resolution 2. Personal equation: colour blindness; slow reaction time; expectation; anticipation; hallucination	1. Mistakes in applying the theory of error to the collection of data and errors in assessing systematic errors 2. Misunderstanding the result: error of interpretation; fallacious reasoning in either the induction or deduction method
Case-studies	1. Millikan vs. Ehrenhaft: measurements of the charge of the electron. (Inappropriate use of Stokes' law led Ehrenhaft to believe in the existence of sub-electrons.)	1. Hertz's experiment on the deflection of cathode rays in an electric field. (Poor vacuum allowed an ionization process to take place which in turn interfered with the electric field.)	1. The canals of Mars. (Physical and physiological limitations coupled with psychological expectation formed a condition for imagining canals.) 2. Blondlot's N rays. ('The observer should look at the screen just as an impressionist painter would look at landscape.')	1. Franck's and Hertz's experiment on the quantized spectrum of the atom's energy levels. (Excitation potential was taken to be an ionization potential.) 2. Herschel's discovery of the planet Uranus. (The observation was initially misinterpreted as a discovery of a new comet.)

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